MEMOIRS OF THE Royal Society;

Or, a New ABRIDGMENT of the

Philosophical Transactions.

Giving an ACCOUNT of the Undertakings, Studies, and Labours of the LEARNED and INGENIOUS in many considerable Parts of the World; from the first Institution of that ILLUSTRIOUS SOCIETY in 1665, to 1740.

In the Course of this WORK, every Thing is carefully extracted from the ORIGINALS, according to the Order of Time; the LATIN TRACTS Englished; the Terms of ART explained; the Theoretical PARTS applied to Practice; and the whole Illustrated with a great Number of COPPER PLATES.

A PERFORMANCE of general Use for the Knowledge and Improvement of MATHEMATICKS, NATURAL PHILOSOPHY, TRADES, MANUFACTURES, ARTS, *&c.*

By Mr. BADDAM.

The SECOND EDITION.

VOL. VII.

LONDON:

Printed for JOHN NOURSE, opposite *Katherine-Street*, in the *Strand*.

M.DCC.XLV.

MEMOIRS
OF THE
ROYAL SOCIETY;

Being a New ABRIDGEMENT of the

PHILOSOPHICAL TRANSACTIONS.

An Account of the Phænomena *of a very extraordinary* Aurora Borealis *seen at* London; *by Dr.* Halley. Phil. Transf. N° 363. p. 1099.

ON *Tuesday Nov.* 10. 1719 about five o'clock in the morning, Dr. *Halley* espied certain white streaks in the sky, that seemed nearly perpendicular; and which, whilst he considered them, appeared instantly to vanish, and soon after others as instantaneously to succeed them: He began to imagine, that this probably was to be some part of the phænomena of the *Aurora borealis*, only there appeared nothing like that luminous arch, which was so often seen in the north; till looking up towards the zenith, he espied an entire canopy of such kind of white *striæ*, which seemed to descend from a white circle of faint clouds, about 7 or 8 degrees in diameter, which circle would sometimes vanish on a sudden, and as suddenly appear again: The Dr. observ'd, that the center of this place of concourse was not precisely in the zenith, but

rather 14 degrees to the southward thereof; which he was well enabled to estimate by a star, that on each return thereof shewed itself about the center of the circle: This star is the 33d of the great bear in *Tycho*'s catalogue, whose distance from the pole at that time was 52° and ½, and which about ½ an hour after five that morning pass'd the meridian; so that those rays centered very nearly on the meridian itself: It was a very agreeable sight, till such time as day-break began to obscure these lights, which were but faint tho' sufficiently distinguishable; none of them came lower than to about 30 or 40° of altitude, and they seemed not to have ascended from the horizon. The sky was perfectly serene and calm; which seems to be one of the concomitant circumstances, attending the *Aurora borealis*, of which this was certainly a species; for, the night following a strange streaming of light was seen in the air, which the Dr. observed from ½ an hour after 9 till 11 o'clock, when there came so thick a fog as to put an end to his prospect: But during all that time there ascended out of the E. N. E. and N. E. a continued succession of whitish *striæ*, arising from below; and after changing into a sort of luminous smoke, passed over head with an incredible swiftness, not inferior to that of lightning; and in some part of its passage seemed gilded, as it were, or rather as if the smoke had been strongly illuminated by a blaze of fire below: Some of the *striæ* would begin high in the air; and a whole set of them subordinate to each other, like organ-pipes, would present themselves with more rapidity than if a curtain had been drawn from before them; some of which would die away where they first appeared, and others change into a luminous smoke, and pass on to the westward with an incredible swiftness; and the Dr. is of opinion, that had it not been for the moon, then 10 days old and very bright, this, for the time, would have been as considerable an appearance, as that of the 6. of *March*, 1716.

An Account of the same Appearance, seen at Cruwys Morchard *in* Devonshire; *by Mr.* Maunder. Phil. Trans. N° 363. p. 1101.

ON the 26. of *October* 1719, between 7 and 8 o'clock in the evening Mr. *Maunder* observed some small appearance of a surprising light (resembling those described in a former *Transaction* and seen on *March* the 6. 1716.) *viz.* 3 or 4 considerable coruscations in form of pyramids, of a reddish colour inclining to yellow, which rose about 50 degrees above the horizon, and continued but a few minutes: But the north part of the hemisphere

ſphere was very bright and red all the evening, both before and after till 10 o'clock, if not longer.

On the 10. of *November* theſe lights were ſeen again about four in the morning, of which ſome ſaid, that the element open'd (as they termed it) ſometimes at one place, and ſometimes at another; from whence iſſued great ſhining lights that continued a while, and then vaniſhed by degrees, and the holes cloſed up again: This continued till day-break.

The following evening Mr. *Maunder* coming from *Tiverton* about ½ an hour after eight, he obſerved the north part of the horizon very bright and reddiſh (notwithſtanding the moon being about 10 days old was then in or paſt the meridian, and ſhone very bright) in a ſhort time the ſtreaming luminous rays began to appear very plainly, ſome in one ſhape, and ſome in another; ſeveral of them reſembling cones or pyramids, but moſt of them ill terminated; ſome of which mounted very high, almoſt to the zenith, to which place or near it, they all or moſt of them ſeemed to point: Soon after there appeared a long ſtreak about 30 degrees, parallel to the horizon, and about 15 or 20 degrees diſtant therefrom and about two or three degrees broad; but ill terminated and of a fiery red colour, which emitted ſome of the ſame ſtreaming beams towards the zenith: About 6 or 7 minutes after there appeared (ſomewhat ſuddenly) a circular figure like an *Iris*, but twice as broad, and of a pale colour: The eaſt part was terminated by the horizon at full eaſt, if not ſomewhat to the ſouth, and the weſt end about north-weſt; the upper part of its arch being 50 or 60 degrees high, a great many luminous rays darted therefrom upwards and downwards (or elſe croſſing it from the horizon) at oblique angles pointing to the zenith, eſpecially, from the north eaſt part: This continued, as near as Mr. *Maunder* could conjecture, about 8 or 9 minutes, when it divided and diſappeared: After an interval of 3 or 4 minutes, another *Iris*-like figure appeared (of a colour, as it ſeemed, paler than any of the ſtreaming lights had been) whoſe diameter was leſs than that of the former, and ſhewed more than a ſemicircle above the horizon, the upper part of its arch approaching near the zenith; he could not obſerve any rays to paſs from, or a-croſs this, as from the other; the center of this laſt was much more to the weſt than that of the firſt: After continuing about a minute or two, it began to break in the upper part of its arch, and ſhining particles, emitted from both its broken ends towards the zenith (to which they were near before) or rather a little beyond it to the ſouth or ſouthweſt, there formed a ſort of *corona*, incurvated

vated somewhat like flames reverberated on the arch of an oven: It seemed to Mr. *Maunder* and others to be finely tinged with various colours, red, yellow and blue, *&c.* and emitted every way, excepting south and south-west, long flame-coloured rays: After this had continued about two minutes, its shining light abated, and it left behind it for some minutes something like a whitish light, and in colour resembling that which the light on the 19. of *March* 1719 left behind it, after the fiery particles were extinguished, but thinner.

It is to be observed, that all this while the moon shone very bright, from which this *corona* was not very far distant, perhaps not 20 degrees to the north-east: After this there continued to be emitted several fiery coloured or yellowish streaming lights, sometimes more, sometimes less, now here, now there, all along the north part of the hemisphere; but mostly from the north north-east: All this while something like small whitish clouds (which appeared to Mr. *Maunder* to move towards the zenith, or to point a little more southward, but disappeared as they approached the moon) were driven very swiftly and at very short intervals, mostly coming from the east and north-east, but several likewise from north and north-west: Mr. *Maunder* took but little notice of this at first; supposing it had been nothing but the reflection of the other lights, or the shadows of the clouds (which the north parts were pretty full of) as the streams of light passed behind them; but at last he observed, that when the lights abated at any time, this kind of clouds continued to fly as swift and frequent as ever. This he observed till 12 or 1 o'clock next morning; several others saw it next morning till almost break of day, when it appeared much more red and fiery than it was in the evening, the moon being then, probably, set: Some people observed tall cones to arise in the east, and to be carried to the west pretty swiftly in an erect position.

A farther Account of the same appearance, as seen at Dublin *in* Ireland. Phil. Transf. N° 363. p. 1104.

ON the 10. of *November*, 1719. at *Dublin* there were surprising lights observed in the northern semicircle of the horizon: The afternoon was very calm and serene; about six o'clock in the evening the sky was tinged with a strange kind of light, and some streams began to project from the north and north-east; one of them arose about north by east, and was nearly a subtense of an arch between that and south west by west; it was somewhat incurvated towards the sun; and what was seen of

of it (for, the north part of the horizon was concealed by houſes) very much reſembled the tail of a comet: About the ſame time there was one or two that aroſe in the eaſt, aſcending obliquely, ſo as to leave the zenith ſeveral degrees to the northward.

Theſe *ſtriæ* continued to appear and diſappear alternately till towards 8 o'clock in the evening; they were pyramidal, and their *vertices* frequently projected ſeveral degrees to the ſouth of the zenith.

Between 9 and 10 o'clock this Gentleman was agreeably ſurpriſed with a kind of coruſcation or flaſhing, that ſhewed itſelf between 20 and 60 degrees from the zenith, in the ſouth or ſouth by weſt; and which from 4 or 5 and ſometimes from more places at once, darted with a velocity not much inferior to that of lightning; and by interfering with each other produced a beautiful tremor or undulation in that ſubtile vapour, which reſembled the beams of the ſun, reflected on a cieling from the ſurfaces of two or three baſons of water: Theſe undulations of light were only viſible at the inſtant of coruſcation, and were of a pale whitiſh colour, ſomewhat reſembling the flaſhes, produced by the violent agitation of mercury in an exhauſted receiver; but ſo ſtrong, that a Gentleman, who about that time happened to be in a room without a candle, took it for common lightning: Thus it continued inceſſantly for more than an hour; during which time ſeveral lucid area's, like ſmall clouds, diſcovered themſelves in the pure ſky; and after they had continued about five or ſix ſeconds of time, as near as could be conjectured, would inſtantaneouſly diſappear; moſt of them pretty much reſembled a very thin white ſmoke or vapour illuminated by the full moon.

About ¾ after 10 o'clock, this vapour was almoſt ſpent, or by a briſk gale at ſouth by weſt diſſipated and driven to the northward; at which time between the weſt and north a vaſt body thereof, reſembling a very bright flame-coloured *crepuſculum*, ſeemed to be fixed: From this baſis ſeveral beams or *ſtriæ* of ſhining matter were emitted at uncertain intervals; and tho' it was not ſo ſenſible to the eaſtward of the north, yet ſeveral huge pillars were alſo emitted from thence; one of which aroſe directly under the pole, and both as to its bulk and denſity ſo far exceeded all others that had preceeded it, that the ſmalleſt print might have been read by the light thereof, had not that of the moon (which ſhone very bright) pretty much effaced it: It was tinged with a kind of yellow and violet colour: In about two or three

three minutes time it died away, and was ſucceeded by others of an inferior order. It was now about $\frac{1}{4}$ after 11 o'clock, when nothing but repeated phaſes of the ſame appearance preſented themſelves to view; the vibrating motion had ceaſed; the vapour ſhewed itſelf no longer in lucid area's; the ſtreams of light were not ſo frequent, and thoſe more languid than before; and the bright *aurora* having ſettled near the horizon, this Gentleman concluded the ſcene was at an end, and accordingly gave over the queſt of new phænomena, only he obſerved, that about N. E. there appeared ſome clouds that reflected an unuſual kind of reddiſh light.

On the 24. of *Nov.* the ſame phænomena were repeated, tho' not with the ſame variety: About $\frac{1}{4}$ after 10 o'clock at night, a vaſt body of ſhining matter was collected between N. W. by W. and N. by E. in the form of a ſegment of a circle, whoſe center was about 25 or 30 degrees below the horizon; from its periphery a few ſhort pyramidal ſtreams of the ſame luminous vapour aſcended by a ſlow and nearly uniform motion, and were exceeding rare, ſo as not to efface the ſmalleſt of the fixed ſtars, and in a minute or two vaniſhed: It was very remarkable, that the light which that collection of vapour emitted was ſo great, that in the otherways dark night, this Gentleman could thereby (at $\frac{3}{4}$ after 10 o'clock) read the title of the *Phil. Tranſ.* which then happened to lie on his deſk; and at four or five yards diſtance ſee the ſmalleſt books in his ſtudy.

An Account of another very conſiderable Aurora borealis *obſerved at* Sreatham *in* Surry; *by Mr.* Hearne. Phil. Tranſ. N° 363. p. 1107.

MR. *Hearne*, having ſeen Dr. *Halley*'s account of the coruſcations in the evening and morning of *November* 10. 1719. found the obſervations made on that appearance very agreeable to what he himſelf had obſerved the evening of that day and to what he did not obſerve at that time, but had an opportunity of obſerving in the night of *December* 11. much more plainly than Dr. *Halley* had in the night of *November* 10.

December 11. about one o'clock at night (or rather in the morning of *December* 12.) he obſerved coruſcations of a much different colour, and in a very different manner from any he had ſeen before.

The ſtreams of light that darted upwards from the horizon ſeemed to be at a conſiderably greater diſtance; but not at all in leſs quantity than thoſe of *Nov.* 10. But their meeting in

in a point near the zenith, and forming there a kind of canopy, was what was particularly remarkable in the manner of the coruscations, now different from those of *Nov.* 10.

The streams of light rose from the horizon only towards the north, and on each hand towards N. E. and N. W. but near the zenith a canopy was form'd of streams of light, meeting in a point, not only from those quarters, but also from the south, *&c.* only they extended downwards from the zenith but a little way to those points, and were neither in so great quantity, nor quite so bright as those northwards: At first Mr. *Hearne* took the point in which the streams met to be exactly the zenith; but upon observing it something longer, he found it a few degrees to the south of the zenith: The streams of light near the zenith, that form'd this canopy, were of a pretty bright colour, and in great quantity darted very swiftly.

On each side of the north towards E. and W. (but not exactly in the north itself) from about 10 or 15 degrees to 40 or 50 above the horizon, the streams were of a glowing red colour; whereas all he had seen before were very pale: The redness resembled that of a burnt brick, and remain'd for a few minutes, like that track thro' which the meteor pass'd in the spring.

The streams appeared of this fierce colour, when Mr. *Hearne* first observ'd the coruscations, and continued so for some time, till the redness wearing off by degrees, in about a quarter of an hour they appear'd of the usual paleness, when he left them still forming a canopy near the zenith, as above described.

The air was very calm and serene, not a breath of wind stirring; as it was likewise *Nov.* 10.

The moon was now a day or two older than it was *Nov.* 10. and a good deal farther to the west than when he observ'd the coruscations that night, being then near full south; she had now round her what is commonly call'd a burr, and larger than ordinary, and several very lucid clouds at a little distance.

Astronomical Observations for the Year 1719. Phil. Trans. N° 363. p. 1109. *Translated from the* Latin.

OCTOBER 10. 1718 in the morning *Jupiter* applied to the telescopic stars, whose places, Mr. *Pound* (on occasion of the first appearance of the comet in 1680, of which *vide Phil. Trans.* N° 342) diligently enquired into, and which being verified he communicated to the Royal Society, together

with an accurate obſervation of a near tranſit of *Jupiter* and again another *February* 11. immediately after the oppoſition of the ſun and *Jupiter*.

In the beginning of *Jan.* 1719 the places of the ſtars were as follows.

	Long.	N. Lat.		Long.	N. Lat.
d	♌ 29° 59′ 43″	1° 7′ 50″	*a*	♍ 25′ 41″	1° 28′ 54″
e	♍ 0 6 13	1 10 18	*x*	♍ 5 43	0 51 56
c	♍ 0 3 13	0 32 50			

Where it is to be obſerv'd, that the ſtars *d* and *e* have preciſely the ſame declination this century; and that *x* is a very ſmall ſtar, and for that reaſon omitted in the former deſcription.

On the 9. of *October*, 1718. 17h. 50′ equated time, *Jupiter*'s eaſtern limb touch'd the line that joins *e* and *c*; and at the ſame time his centre was diſtant from *e* 21′ 20″, and from *c* 16′ 25″, and from *d* 19′ 35″; the ſmall ſtar *x* being near *Jupiter*, was hid and obſcured by his light.

On the 11. of *December* 18h. 30′ equated time, *Saturn*'s centre was diſtant from *Bayer*'s μ in *Libra* 28′ 32″, and more northerly than that ſtar 4′ 31″: Hence Mr. *Pound* concluded, that *Saturn*'s place was ♏ 10° 41′ 10″, with 2° 16′ 43″ N. Lat.

On the 11. of *February*, 1719, 6h. 56′ and ½ equated time, the centre of *Jupiter* (then retrograde) was diſtant from the ſtar *d* 10′ 42″.

At. 6h.	58′	and ¼	The ſame center was diſtant from *e*	6′ 7″
9h.	37	and ½	The diſtance again taken from *d*	10 9
9	43	and ½	Again from *e*	6 11
9	49	and ½	*Jupiter*'s centre was diſtant from *a*	25 21
9	58	and ½	From the ſmall ſtar *x*	24 38

About 7h. *Jupiter*'s eaſtern limb touch'd a line drawn thro' *x* and *e*; therefore, *Jupiter* was at that time in 6′ of ♍ with 1° 16′ 30″ N. Lat.

On the 13. of *February* 8h. equated time, the declination of *Jupiter*'s center, as meaſur'd by the micrometer, was more northerly than that of both the ſtars *d* and *e* 11′ 37″, and at 8h. 20′ the ſame difference was found 11′ 36″. But at 8h. 48′ *Jupiter*'s center was diſtant from *e* 17′ 40″.

On the 22. of *April* 10h. 45′ equated time, *Saturn*'s centre followed μ of *Libra* 4″ and ½ of time, or 1′ 8″ *R. Aſcenſ.*

But

But he was found by the micrometer to be more northerly than the fix'd ſtar 35′ 25″: But in the *Catal. Britan.* the ſtar was at that time in 10° 16′ 8″ ♏, and 2° 3′ 54″ N. Lat.

On the 16th of *May* 8h. eq. time *Jupiter* followed *Cor Leonis* 1° 34′ and ½ *R. Aſcenſ.* but was more northerly than that ſtar 41″ and ½ of time, that is, 10′ 7″ of an arch of the heavens.

The ſame night at 15h. 18′ app. time Mr. *Stephen Grey* obſerv'd, that *Mars*, as to *R. Aſcenſ.* followed the eaſtern ſtar in *Capricorn*'s tail 16′ 15″; and at the ſame time was but 11″ more ſoutherly than the ſtar.

On the 7th of *June* 10h. 15′ app. time, *Jupiter* being direct, return'd again to the above-mentioned teleſcopic ſtars, and then followed the ſtar *d* 35″ *R. Aſcenſ.* And at 10h. 30′ the fix'd ſtar was diſtant from *Jupiter*'s neareſt limb 4′ 18″.

On the 8th of *June* 10h. 20′ *Jupiter* followed the other ſtar *e* 1′ 30″ *R. Aſcenſ.* and the diſtance of *Jupiter*'s neareſt limb from the ſtar was immediately taken by the micrometer 7′ 30″.

On the 5th of *July* 8h. 26′ app. time, *Jupiter* and *Venus* were in cloſe conjunction; and ſhe being then more northerly preceded *Jupiter* in *R. Aſcenſ.* 1′ 20″. The mean diſtance of their centers from 10 ſeveral obſervations was 13′ 36″. Theſe three obſervations, made at *London*, were communicated by Mr. *Folkes*.

On the 3. of *Auguſt* 12h. 20′ eq. time, *Mars* being almoſt acronic followed the ſtar which preceeds *Bayer*'s τ in *Aquarius* 10′ 58″ of time, or 2° 44′ 57″ *R. Aſcenſ.* and he was but 36″ more ſoutherly than the ſtar: Whence having the place of the ſtar in the *Catal. Britan.* the place of *Mars* will be 7° 10′ 10″ of ♓ with 6° 38′ 10″ S. Lat.

On the 10th of *Auguſt* 11h. 50′ eq. time *Mars* follow'd the leſſer ſtar, which preceeds τ in *Aquarius* 1° 39′ 30″ *R. Aſcenſ.* but was more ſoutherly than the ſtar 10′ 42″.

On the 16th of *Auguſt* 7h. 18′ eq. time *Spica Virginis* preceded *Venus*'s center 5″ and ¼ of time, or 1′ 20″ *R. Aſcenſ.* and was more ſoutherly than the planet 18″ and ¼ of time or 4′ 35″.

On the 17th of *Auguſt* *Mars*, being acronic the day before and near the earth, was obſerv'd near two contiguous ſmall ſtars, the northern of which was in 3° 5′ 50″ of ♓ with 6° 6 and ¼ S. Lat. but the other being more ſoutherly was in

3° 5′ 30″ of ♓, with 6° 10′ and ¼ S. Lat. nearly: But at 10h. 40′ eq. time *Mars* followed the southerly star 41′ 40″ *R. Ascens.* and was still more southerly than the star 7′ 50″.

On the 18th of *September* 9h. 20′ eq. time *Mars* was observ'd to preceed the 53d star of *Aquarius* in *Catal. Britan.* 3′ 45″ of time or 56′ 24″ *R. Ascens.* and at the same time the star was but one diameter of *Mars* more northerly than his northern limb. The place of the star was in 29′ 57″ and ½ of ♒ with 4° 48′ and ½ S. Lat.

On the 30th of *October* 5h. 45′ app. time in the evening, *Mars* was near the two contiguous stars next *Bayer*'s *h* in *Aquarius*, and which are the 73d and 74th of *Aquarius*, in *Catal. Britan.* He had pass'd the streight line drawn thro' them, and the angle form'd at *Mars*'s center was a right angle as to sense: The northerly of the two stars had the same declination with the southern limb of the planet. At 5h. 53′ the distance of the star from *Mars*'s center was 2′ 30″. At 5h. 56′ *Mars*'s center was distant from the third and more southerly star at *h* or the 75th of *Aquarius* 17′ 4″. At 6h. 18′ his center was distant from the northern or 73d star 3′ 5″: Hence we may conclude, that *Mars* was in conjunction with the northern star at 5h. 30′ nearly, and had left it only 1′ to the north. The place of the star by the *Catal. Britan.* was at that time in 10° 29′ of ♓ with 1° 40′ and ¼ S. Lat. The 74th star was in 10° 29′ 50″ of ♓ with 1° 44′ and ¼ S. Lat.

On the 16th of *November* 19h. 18′ eq. time, *Venus* preceeded the southern scale of *Libra* 3′ 13″ of time, or 48′ 23″ *R. Ascens.* and at the same time the center of the planet was more northerly than the fix'd star 7′ 45″. *Venus* was stationary, as it were, at her ascending node.

On the 3d. of *December* 19h. eq. time *Saturn* preceeded the third star near ζ of *Libra*, or the 29th star of *Libra* in *Catal. Britan.* 46″ of time or 11′ 32″ *R. Ascens.* But he was more southerly than the fix'd star 15′ 29″, the difference being taken by the micrometer: Whence *Saturn*'s place was in 20′ 25″ and ¼ of ♏, with 2° 5′ and ¼ N. Lat.

In *Phil. Trans.* N° 357, there is an account of a lunar eclipse, observ'd at *Cambridge* in *New England* on the 15th of *March p. m.* 1717 O. S. but in *England* it could not be seen by reason of clouds; the eclipse ended there at 11h. 42′ and ½, which was the only observation had of it at that time: But afterwards Admiral *Candler* brought from *America* and com-

communicated the phases of that eclipse, observ'd at *Lima* in *Peru* by S. *Petro Peralta*, Professor of Mathematics, and publish'd there.

The eclipse began at *Lima* at 8h. 41′ 8″, and ended at 11h. 19′ 55″.

Admiral *Candler* himself observ'd at the island of *Virgine Gorda* the eclipse end at 12h. 13′ *p. m.* the sky being then very clear.

In the Memoirs of the Royal Academy at *Paris* for the year 1717, there are two observations of this eclipse agreeing pretty well with each other, the one by M. *Cassini* and the other by M. *De la Hire* in the Royal Observatory at *Paris*: The latter estimated the beginning at 13h. 54′; but the end with greater certainty at 16h. 38′ 10″: The former estimated the beginning at 13h. 55′, and the end at 16h. 38′ 25″: The greatest obscuration according to M. *De la Hire* was seven digits and ⅓; and according to M. *Cassini* seven digits and ½.

Hence, the end having, as it seems, been observ'd at each of the said places with greater accuracy, the differences of Long. of *Paris* and *Lima* will be 5h. 18′ 20″; between *Paris* and *Cambridge* in *New England* 4h. 55′ 50″; between *Paris* and the island of *Virgine Gorda* 4h. 25′ 20″: From which if you substract 9′ 40″, the Long. of *Lima* to the west of *London* will be 77° 10′; of *Cambridge* in *New England* 71° and ½, and lastly, of *Virgine Gorda* 63° 55′: Whence the geographical situation of the neighbouring islands may be corrected with certainty.

On the 9th of *September* 1717 in the evening another eclipse of the moon was observ'd at *Paris* by the same gentlemen and M. *Maraldi*; at *London*, the end was observ'd at the Royal Society's house at 7h. 26′; at *Paris* M. *Cassini* observ'd it at 7h. 34′ 50″; M. *Maraldi* at 7h. 35′ 30″; and M. *De la Hire* at 7h. 34′ 15″; and at the same time at *Norimberg* M. *Wurtzelbaur* observ'd it at 7h. 10′ 45″: Hence, especially by *Maraldi*'s observation, is confirm'd the difference of meridians between *London* and *Paris*, *viz.* 9′ 30″; as also between *London* and *Norimberg* 44′ 45″, as we have often found it before: Moreover, on the fifth day after the eclipse, to wit, the 14th of *September* in the evening, at *Paris* the moon hid *Palilicium*, being accurately observ'd there by M. *Maraldi* and M. *De Lisle* the younger; at 9h. 11′ 35″ the star disappear'd over against the *Macula Grimaldi* or *Palus Mareotis*; but at 10h. 3′ 55″ it emerged out of the obscure limb of the moon:

moon: As to the observation of this occultation at *London* vide *Phil. Trans.* N° 357.

These observations, in which equated time is applied, were communicated by Mr. *Pound*; and being taken with a 15 foot telescope, may be depended upon as very accurate.

Some Remarks on an Essay of M. Cassini's, *wherein he proposes to find by Observation the* Parallax *and Magnitude of* Sirius; *by Dr.* Halley. Phil. Transf. N° 364. p. 1.

IN the Memoirs of the Royal Academy of *Paris* for the year 1717, there is a very remarkable essay by M. *Cassini*, concerning the annual parallax of the fix'd stars, and particularly of *Sirius*; and in conclusion, he determines the diameter of *Sirius* to be as much bigger than that of the sun, as the diameter of the sun is greater than that of the earth, which he supposes to be 100 times: Now the distance from the sun to the earth being certainly about 100 diameters of the sun, it will follow, that the globe of *Sirius* must be a sphere, whose diameter must equal the distance between the earth and sun.

To prove this, he tells us, that he made use of an excellent telescope of 34 *French* feet or 36 *English*, with an aperture of but one inch and a half, to take off the spurious rays of the star, which then appeared round and sufficiently well defin'd; and comparing his body to that of *Jupiter* (which he says was then 50″ diameter) he found the diameter of *Jupiter* to be 10 times greater than that of the star, which, consequently, was seen under an angle of about 5″; which is M. *Cassini*'s first position.

Then he tells us, that to make the observations of the parallax of this star with all the exactness possible, he made use of a telescope of three foot in a copper tube, having fixed, in the common *focus* of the two glasses, four threads crossing each other in the center under angles of 45 degrees: This tube he firmly fix'd to the plane of a mural arch, which had been upwards of 30 years immoveably cemented to the wall of the Royal Observatory; to which he chose to fix it, because of its solidity, and its being, therefore, the less liable to shake; for, having stood 30 years, there was no fear of its settling any farther in the space of one year; besides, it might easily be perceived if any such alteration should happen to it.

M. *Cassini* having, therefore, fix'd his three foot tube as above; so that about the beginning of *April* 1714, N. S. (Dr. *Halley* supposes, because *Sirius* was at that time in square

to

to the ſun) the ſtar being exactly in the meridian, paſs'd over the center of the tube, he obſerv'd, that on the 20th of *April* the ſtar touch'd the horizontal thread with its under edge, being apparently all above it in the inverting tube, but really below it. On the 15th of *May* and 6th of *June* it paſs'd again by the center. On *June* 27. it appear'd a little under; and on *July* 9. it was found to touch the under part of the thread: On *October* 5th it again paſs'd by the center; but on *December* 29th it touch'd the upper part of the thread: *January* 18th 1715, the coldeſt day of that winter, it paſs'd exactly by the center; and on the 27th of *March* and the 1ſt of *April* it almoſt touch'd the upper ſide of the horizontal thread, from which it ſeemed a little ſeparated: But on *June* 7th it paſs'd a little under the center; and on *June* 29th the ſun being then in conjunction with *Sirius*, it paſs'd under the thread, ſo as to touch it with its upper edge: Whence it appears, that in the ſpace of a whole year, there had been no other variation of the meridian altitude of *Sirius* than the breadth of the thread, which appeared equal to the diameter of the ſtar, which M. *Caſſini* takes to be five, or at moſt ſix ſeconds.

Suppoſing this to be ſo, M. *Caſſini* then ſhews, that the whole diameter of the annual orb is to the diſtance of *Sirius*, as the ſine of 6″ to the ſine of 39° 33′ the Lat. of the ſtar: Whence the aforeſaid immenſe magnitude of its body is a neceſſary conſequence.

But before this be entirely admitted; it may not, perhaps, be amiſs to enquire, whether the ſuppos'd viſible diameter of *Sirius* were not an optic fallacy, occaſioned by the great contraction of the aperture of the object glaſs: For, we all know, that the diameters of *Aldebaran* and *Spica Virginis* are ſo ſmall, that when they happen to immerge on the dark limb of the moon, they are ſo far from loſing their light gradually, as they muſt do, were they of any ſenſible magnitude, that they vaniſh at once with their utmoſt luſtre, and likewiſe emerge in a moment, not ſmall at firſt; but at once appear with their full light, even tho' the emerſion happen very near the cuſp; whence if they were four ſeconds in diameter, they would be many ſeconds of time in getting entirely clear of the limb: But the contrary appears to all thoſe that have obſerv'd the occultations of theſe bright ſtars; and tho' *Sirius* be bigger than either of them, yet he is by far leſs than two of them; and conſequently, his diameter to theirs is leſs than the ſquare root of 2 to 1, or than 14 to 10: whence, in M. *Caſſini*'s excel-

excellent 36 foot telescope, those stars ought to be about 4″ in diameter; and they would undoubtedly appear so, if view'd after the same manner; whereas, we are otherways assur'd, that they are less than a single second in diameter; the great strength of their native light forming the resemblance of a body, when it is nothing else but the spissitude of their rays.

As to the other part of the argument, to wit, that the alteration of the declination of *Sirius*, on account of the access of the earth in *December* and its recess in *June*, amounts to 6″; Dr. *Halley* only remarks, that, besides that a radius of three foot, such as M. *Cassini*'s seems to have been, is somewhat too small for so extremely nice an observation; 6″ being subtended by the $\frac{1}{1000}$ part of an inch; some of the above-mentioned observations plainly shew, that the refraction of the medium did intermix with those differences, that might be occasioned by the parallax.

But the principal objection against the conclusion of this argument, seems to be, that the meridian altitude of *Sirius* at *Paris* being under 25 degrees, the ordinary refraction of the star is 1′ 55″ or 115 seconds; and the barometer rising and falling upwards of two inches in 30, shews, that the density of the air, on that account, may be $\frac{1}{15}$ part more at one time than another: Whence the refractions being always proportional to the density of the medium, as it has been often demonstrated by Mr. *Hauksbee*, both *in vacuo*, and in a twice or thrice condens'd air; it is plain, that in that altitude, the refraction of a star may differ about seven or eight seconds or $\frac{1}{15}$ part of 115″, which is more than the whole parallax, suppos'd to have been observ'd.

It were to be wish'd, that M. *Cassini* would try this matter by the *Lucida Lyræ*, instead of *Sirius*, which tho' somewhat less than him, is as near to the solstitial colure, and has much greater Lat. being but 28 degrees from the pole of the ecliptic, whence its parallax would be so much greater; and at *Paris* being within 10 degrees of the zenith, the grand objection of the difference of refraction would be almost entirely remov'd.

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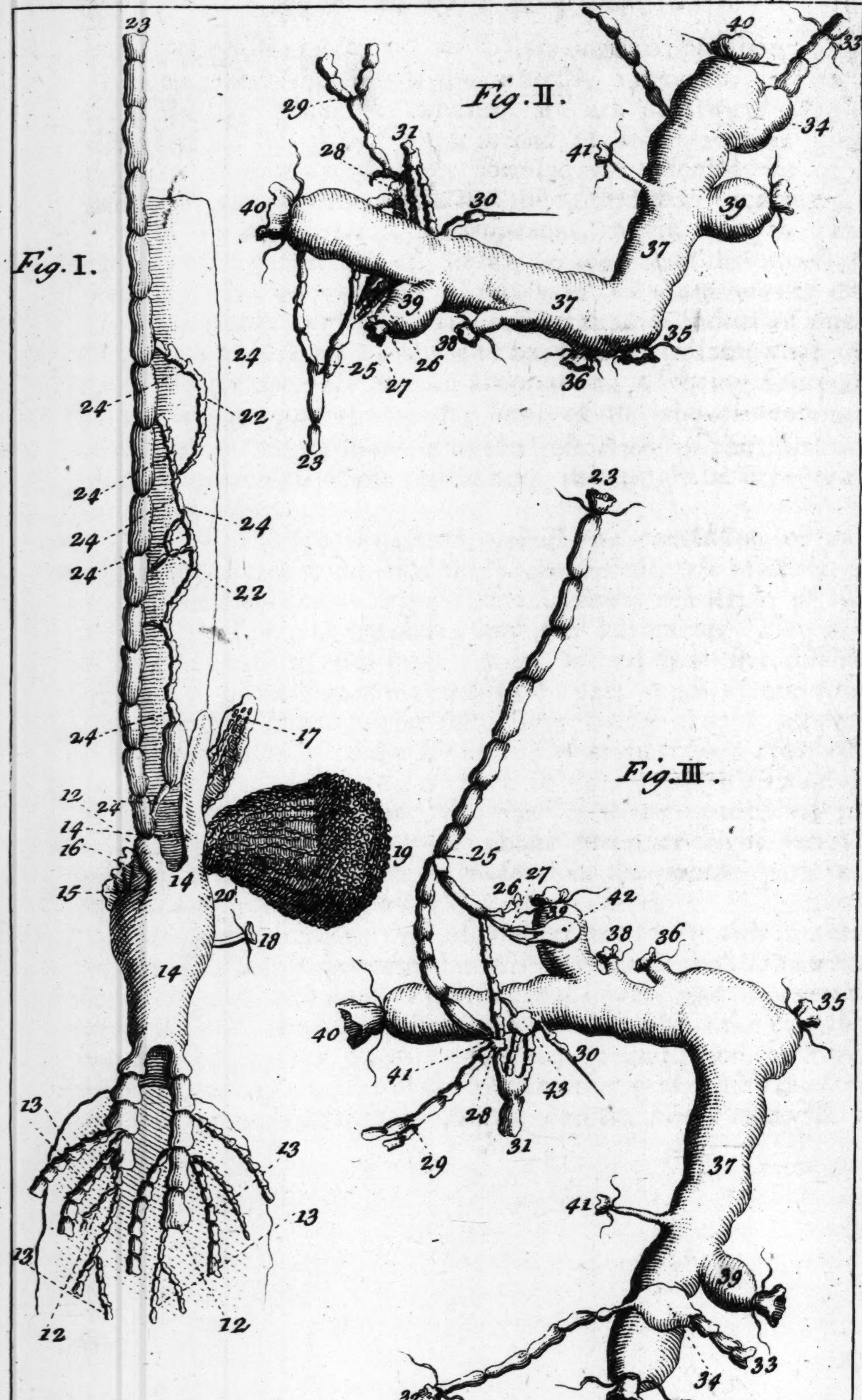

G. Smith del. Hulett Sculp.

An Account of the external maxillary and other ſalival Glands; *as alſo of the inſertions of all the* Lymphatics (*both above and below the ſubclavians*) *into the* Veins; *by Dr.* Rich. Hale. Phil. Tranſ. N° 364. p. 5.

THE external maxillary glands in brutes are of the conglomerate kind; they lie externally, laterally, lengthways, on the lower jaw, partly under the *depreſſor labiorum*, and partly under the *buccinator*: A ſtrong membrane intervenes between theſe glands and the jaw on one ſide, and between them and the buccal glands on the other ſide: They are more or leſs red, like the *pancreas*, according to the quantity of blood remaining in them; otherways their ſubſtance is white.

Theſe glands receive arteries from the external carotids; veins from the external jugulars, and nerves from the third branch of the fifth pair.

The number of excretory ducts from theſe glands is not always the ſame in the ſame ſpecies of animals: In cows generally 14 are diſcovered by the probe; their orifices are valvular, about four times leſs than their ducts; every duct is about ½ an inch from the next; thoſe in the middle of the glands are largeſt, becauſe there the glands are broadeſt and thickeſt: The ducts neither communicate with each other or with the buccal; every duct is formed of leſſer ducts united together, which riſe from the lobules (throughout the whole ſubſtance of the glands) which conſtitute each diſtinct lobe, and has the ſame ſtructure with the pancreatic duct; each lobe is depreſſed on its ſides, where it is joined to other lobes; and between the lobes ſeveral buccal glands are interſperſt.

In calves ſeldom more than 6 or 7 ducts admit any probe; when the animal grows older, the ducts appear more plain and open.

In ſheep ſix excretory ducts are always found in each external maxillary gland.

In dogs and cats, &c. theſe ducts are fewer, in proportion to the ſmallneſs of the glands: It is obſervable, that theſe ducts in dogs open obliquely towards the mouth, whereby the *ſaliva* may be the better mixed with the food in maſtication; which from any other ſtructure of ducts in theſe animals, that ſwallow greedily might be ſwallowed unmixt.

Dr. *Wharton cap.* 21. is the firſt who mentions the external maxillary glands; what he ſays of them is only applicable to their appearance in human ſubjects, where they are of the con-

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glomerate kind and very ſmall, unleſs in ſcrophulous and venereal caſes: It is plain, that he had not ſeen them in brutes; for, in his figures (which were drawn from brutes) no notice is taken of theſe glands: He deſcribes them as very ſmall, and calls them emunctories of the nerves; which was the notion in his time concerning the uſe of the conglobate glands; and the *ſaliva* was ſaid, as in *cap.* 21. *p.* 134. *è nervoſo genere profundi.*

Steno in *Obſ. Anatom. p.* 14. juſtly cenſures *Blaſius* for aſcribing to the external maxillary glands an excretory duct opening into the mouth, like the common duct from the parotid gland: Yet *Steno*, otherways very accurate, does not truly deſcribe theſe glands, nor diſtinguiſh them from the buccal, tho' they are as diſtinct from theſe, as the ſublingual are from the external maxillary glands: *Steno* divides his buccal glands into three parts: The large ducts riſe in a line from the external maxillary glands; and how diſtinct theſe glands are from the buccal appears plainly in Fig. 4. Plate II. *&c.* *Steno*'s ſecond part of the buccal glands p. 18. *intra quæ & in mediâ parte*, are mark'd *e e* in Fig. I. *quæ alias*, &c. higher are the ſame *e e* among the *papillæ*; the third part, *quæ a ſuperiore deſcendunt*, are *a b c d.*

The external maxillary glands differ from the buccal, in bigneſs, figure, ſtructure, particular number of ducts, colour, *&c.* The buccal, labial, internal maxillary and ſublingual glands are of a yellow colour; beſides, the buccal are ſeparated from the external maxillary by a ſtrong membrane: It is true, that ſeveral of the excretory ducts of the buccal glands open near the ducts of the maxillary (whence *Steno* confounded theſe glands) but they likewiſe do ſo round his own ducts from the parotids; and ſome ducts from like glands open near the ſublingual, as alſo about *Nuck*'s ducts; in which places the buccal ducts are moſt numerous.

In ſhort, there is a very great number of excretory ducts diſperſed all over the membrane, which inveſts the mouth, *fauces*, *&c.* which riſe from glands that lie under this internal membrane: Theſe glands are more numerous in ſome parts than others, and receive different names according to the part they belong to, as labial, buccal, palatine, *&c.* But theſe are ſmall glands with one excretory duct, and tho' they ſeparate *ſaliva* like the large conglomerate glands, parotids and maxillaries, *&c.* yet they differ from theſe in ſtructure, one common excretory duct, *&c.* whereas the external maxillaries differ from all the other glands of the mouth, *viz.* from the buccal in ſeveral reſpects, beſides their colour; in which particular they are alſo diſtin-

diſtinguiſhed from the internal maxillary and ſublingual glands; they likewiſe differ from theſe as well as from the parotids, in having a great number of common excretory ducts: This number of excretory ducts was not obſerved by *Steno*; nor did he know, that theſe ducts in the ſame line were the excretory ducts of large conglomerate glands (like the parotids) diſtinct from the buccal.

Bartholin p. 542. mentions the external maxillary glands, but does not deſcribe them: *Nuck Adenol.* p. 5 N° 11. only gives them a place in his catalogue of glands; but takes no farther notice of them, tho' he writ a book (*Sialog. p.* 15. 158.) chiefly about a new ſalival duct riſing from a gland, that is found in no other animal beſides a dog.

Mr. *Cowper* had never obſerved theſe external maxillary glands, as appears by a letter of his to Dr. *Hale* in anſwer to one the Dr. ſent him upon the firſt diſcovery of theſe glands: The external maxillary glands of the conglobate kind in men are marked *g* in Fig. 1. of his *Myotomia reformata.*

The ducts of the external maxillary glands are oppoſite to the orifices of *Steno*'s ducts, from which glands and ducts, as alſo from the buccal, labial and gingival glands, the *ſaliva* flows from all parts of the mouth without the teeth; from *Wharton*'s and the ſublingual ducts, from the tonſils, *fauces*, *fretum Stenonis*, gingival, lingual and palatine glands, the *ſaliva* is derived from the upper and lower, fore and hinder-parts of the mouth within the teeth.

What has been ſaid of theſe ſalivary glands, &c. will be beſt underſtood by the following figures, drawn in *October* 1697 at *Trin. Coll. Oxon.* by Mr. *Burghers*, and ſince compared with the parts themſelves in cows, calves, &c. theſe figures are part of many more taken from preparations at the ſame time.

The inſertions of all the lymphatic veſſels into the veins can be diſcovered but in few ſubjects, and no figure has hitherto been given of them.

Theſe figures ſhew the courſe of the *lympha* both below and above the ſubclavian veins in men and axillary veins in dogs. The *lympha* below the *receptaculum chyli* is conveyed from all the inferior parts by a great number of ſmall lymphatic veſſels, which uniting with others obliquely above the valves, become proportionably bigger, till at length they conſtitute two large trunks near the emulgents, which are the *pedunculi* or beginnings of the *receptaculum chyli*: The *lympha* from the parts above the ſubclavian veins is derived in like manner from leſſer lymphatics to the common ducts here delineated.

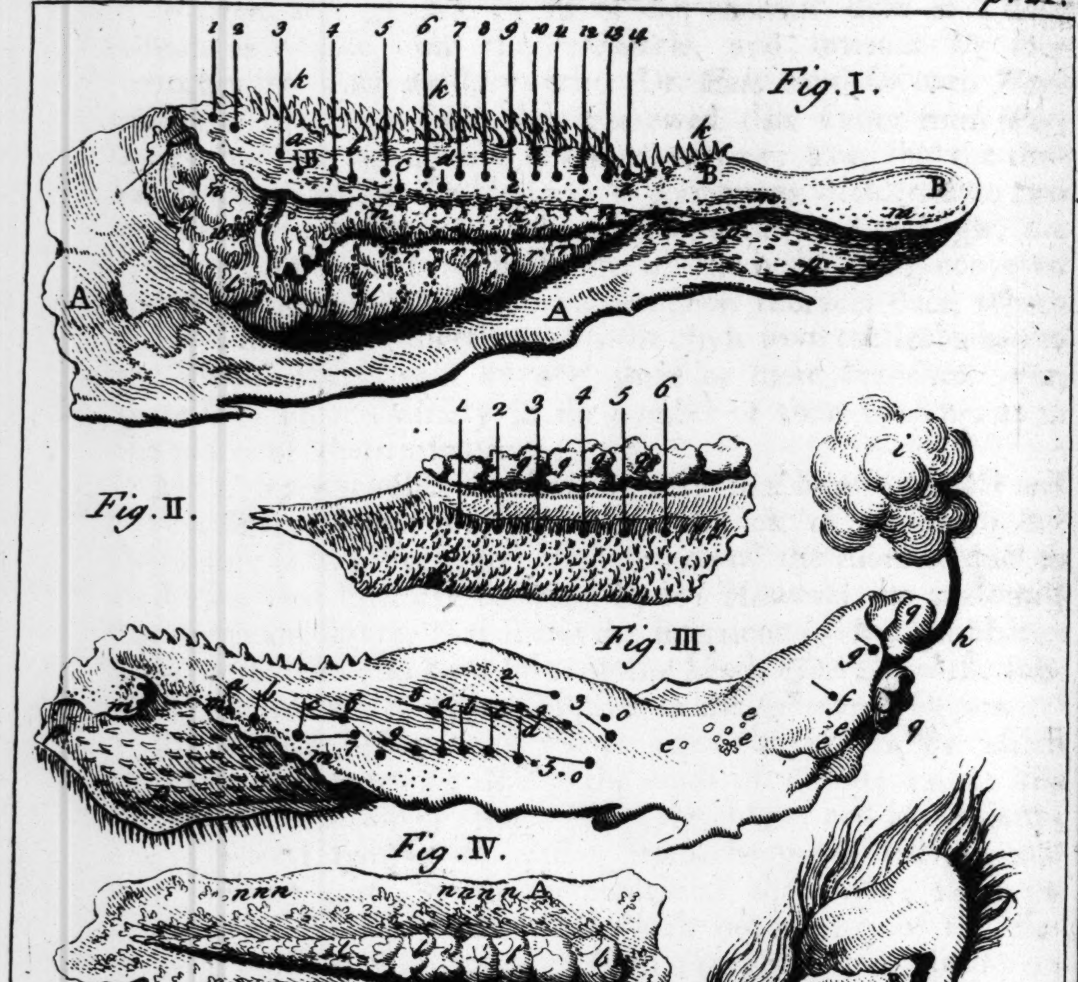
Pecquet has given a figure of the thoracic duct in a dog, which is double from the receptacle, and inserted by four branches into each axillary vein: Dr. *Hale* supposes with *Bartholin* p. 616, 620. (who has borrowed this figure from *Pecquet*) that such an insertion is a *lusus naturæ*: For, tho' the thoracic duct may be double, and is sometimes divided into two parts near the subclavian veins; yet generally, it is single, the *lympha* from all parts on both sides of the body being conveyed by proper *lymphæducts* into one common thoracic duct, which conveys this liquor together with the chyle from the lacteals into the left subclavian vein by one, three or more branches. For, there is as great a variety in the number of these branches as in the places of their insertion.

Mr. *Cowper* injected the thoracic duct in a human subject and gave a figure of that preparation in his book of anatomy: But this figure is imperfect, and the insertion of the thoracic duct so ill drawn, that little can be learnt by it: However, no anatomist has given any figure, that shews the insertions of the lymphatics from both arms and both sides of the head, *&c.* above the subclavian veins, which appear so plain in the following figures.

Fig. 1. Plate I. represents the passages and vessels, by which the chyle and *lympha* pass into the veins of a dog; 12, 12 the lymphatics that convey *lympha* from the thighs and lower parts; 13, 13 lateral lymphatics, arising from the groin, testicles and neighbouring parts; 14 the receptacle of the chyle; 15 an indenture in the receptacle, thro' which one tendon of the diaphragm passes; 16 lymphatics from a neighbouring gland; 17 some lymphatics from the diaphragm; 18 an artery that supplies the loins and runs thro' a division of the receptacle; 19 the *pancreas Asellii*; 20 the *vasa lactea secundi generis*; 21 the beginning of the thoracic duct; 22 some divarications of the duct; 23 the continuation of the duct and its progress; 24 the descending *aorta*.

N. B. That the arteries at 18 and 24 (as also the tendon at 15) by their pulsation do much promote the ascent of the chyle and *lympha*.

Fig. 2. At 25 represents a common divarication of the duct; 26 a lymphatic from some neighbouring gland; 27 a double lymphatic from the secondary gland 42, as in Fig. 3. 28 that part of the thoracic duct, where both its branches, and the lymphatics from the left side of the head and left fore-leg meet; 29 the lymphatics from the left side of the head and left fore-leg united; they lie on the inside of the vein; 30 a lymphatic with a pin



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a pin in it from a neighbouring gland, perhaps the *thymus*; 31 a lymphatic from the neck, &c. it is divided and enters the jugular by two distinct branches under the *sacculus* 43; 32 the lymphatic from the right side of the head; 33 the lymphatic from the right fore-leg; 34 the large *sacculus*, or receptacle of the *lympha*, on the right side, that receives all the *lympha* on that side and conveys it into the jugular vein; 35 the descending *cava*; 36 the mammary vein, which is sometimes single; 37 the subclavian veins; 38 the vertebral vein, 39 the axillary veins; 40 the jugular veins; 41 the right internal jugular not injected; 42 a small secondary lymphatic gland on the back part of the top of the *thorax*, 43 the *sacculus*, that receives all the chyle and *lymyha* from the whole body (excepting 30, 31, 32, 33, 34) and discharges it into the vein; at least we know of no other lymphatics that any where else enter the veins; 44 a lymyhatic (or membrane, for it was not injected) that joins 29 to the largest branch of the thoracic duct.

Fig. 3. represents the upper part of Fig. 2. reversed; the duct, &c. being turned up, that the insertion, both *sacculi*, &c. may be the better discovered; this figure is to be explained by the preceeding; and has only from 42 to 44 more than the upper part of Fig. 1.

N. B. In this subject the chyle and *lympha* are emptied into the jugular and not into the axillary veins; they are sometimes emptied partly into the jugular, and partly into the axillary or subclavian vein; in men, generally into the subclavian.

Fig. 1. Plate II. represents part of the left cheek of an ox separated from the lower jaw-bone, with the external maxillary glands, its ducts, &c. 1, 2, 3, &c. to 14 are bristles inserted into the ducts of the external maxillary gland *lll*; these ducts open sloping into the mouth, for the better mixture of the *saliva* with the food; 15 the duct 3 injected with wax to discover its division and bigness in respect of the orifice; 16 a lobule of the maxillary gland; its excretory duct is injected with wax and ends at 15; 17 the duct 1 laid bare and opened to shew its large cavity, &c. A A part of the muscles and fat, &c. belonging to the lower jaw; B B part of the internal membrane that invests the mouth; *abcd* bristles in those ducts of the buccal glands *nn*, that would admit any; *eee* those orifices of the buccal glands, that were too little to admit bristles; *kkk* the *papillæ* on the inside of the mouth; *lll* the lobes that constitute the external maxillary gland; *mmm* the orifices of the labial glands *pp*, that were too small for passing bristles; *nnn* buccal glandules interspersed

ſperſed between the lobules of the maxillary gland; *nnn* near *rrr*, part of the buccal glands, where they appear thickeſt, being raiſed to ſhew the ducts *rrr*, running under them; *ppp* the labial glands like the buccal; Mr. *Cowper* in his Fig. 4. repreſents them at HH; *rrr* the ducts marked *6* to 14, as they appear under the glands *nn*.

N. B. The ſame numbers and letters repreſent the ſame things in the following figures.

Fig. 2. ſhews part of the left jaw-bone and cheek of a ſheep, where the briſtles 1, 2, 3, *&c.* repreſent the conſtant number of excretory ducts from the external maxillary gland in theſe animals.

Fig. 3. ſhews part of the right cheek of a dog taken from the lower jaw-bone: *f* the orifice of *Steno*'s ſalival duct; *g* the orifice of *Nuck*'s duct, which riſes as a *papilla* on the membrane BB; *h Nuck*'s new duct, not found in men, oxen or ſheep, but in dogs, their orbit not being entirely bony; *i Nuck*'s gland; *ooo* the orifices of ſome excretory ducts, belonging to the external maxillary gland, that were too ſtrait for the admiſſion of briſtles; *qq* the teeth; in this ſubject they are the teeth of the upper jaw, near the ſecond of which the orifice of *Nuck*'s duct appears.

Fig. 4. ſhews the back part (next the *cutis*) of the external maxillary gland of the ſame dog, as it is inveſted with the buccal glands.

Fig. 5. repreſents the external maxillary gland in the right cheek of a cat: In this ſubject the Dr. could only probe two ducts; three *&c.* would not admit briſtles.

An Account of the Plague *at* Conſtantinople; *by Dr.* Timoni. Phil. Tranſ. N° 364. p. 14. *Tranſlated from the* Latin.

IT is manifeſt both from hiſtory and experience, that the plague is communicated from *Egypt* to *Conſtantinople*; and tho' ſome ſeeds of the old infection ſtill remain; yet new matter is at times conveyed: It is pretty well ſuppreſſed by the intenſe cold of the winter; yet here and there ſome ſigns of it appear, both in winter and ſpring; it gathers ſtrength in ſummer, and is at its greateſt height in autumn: Tho' the northerly winds (that blow in ſummer at ſtated times) be here ſomewhat cold; yet they do not ſtop the ſpreading of the peſtilential venom: If the ſoutherly winds, that are pretty warm, do conſtantly blow, they ſuppreſs the plague in ſummer: The ſymptoms

toms of the plague at *Conſtantinople* are exactly the ſame with thoſe of the plague at *Numegen* in 1636, 1637, as deſcribed by *Diemerbroeck*; it ſeiſed on man *cap.* 4. and one or two horſes, dogs or cats were infected with peſtilential *bubo*'s, of which they died. The common people (eſpecially the poorer ſort) among *Turks*, *Chriſtians* and *Jews* are perſuaded that the plague is a judgment from heaven; and therefore, they do not guard againſt the contagion; but the better ſort, eſpecially the *Chriſtians*, take all poſſible precaution.

The ſymptoms according to *Diemerbroeck cap.* 7. are, fevers, *bubo*'s, carbuncles, *exanthemata*, head-ach, phrenſy, ſleepineſs, watchings, anxiety, great weakneſs, diſordered viſion, palpitation of the heart, drineſs of the tongue, vomiting, ſighing, worms, looſeneſs, hemorrhage at the noſe, flowing of the *menſes*, voiding of blood by the *urethra*, ſpitting of blood, pains of the ſide, liver, reins and other parts.

To theſe Dr. *Timoni* adds an ulcerous laſſitude of the joints, ſhuddering, with ſometimes a heat enſuing, and oftener none at all, a *nauſea* without vomiting, *vertigo*'s and vertiginous motions, and a trembling of the hands on the firſt onſet: None of all the above-mentioned ſymptoms is inſeparable from the plague; nay, theſe three, to wit, *bubo*'s, carbuncles and *exanthemata* are not peculiar thereto, and often there is no fever: So that the following general rule may be laid down; *viz.* 'when there are plain ſigns of the plague, we may certainly conclude, the diſtemper to be the plague; but when there are no ſuch ſigns, we cannot certainly conclude the contrary:' For, after ſeveral have been ſeiſed with the plague, and a gentle *rigor* has preceeded, none of the above-mention'd ſigns appear for ſeveral days together, but afterwards they break out plentifully: Some, after catching the infection, are only ſeiſed with a very ſlight languor, and ſoon after they go about their ordinary buſineſs without any inconveniency; but on the third or fourth day they fall down ſuddenly, and expire on the high-way, and after the patient's death black ſpots appear, which remove the ſuſpicion of an apoplexy or any other diſorder; and this is likewiſe confirmed both by the antecedent manifeſt cauſe of the infection, and by the contagion afterwards ſpreading: Several walk about without any manifeſt ailment and are cured, even when the *bubo*'s and carbuncles break out; yet the patients generally fever, and are very bad: Vomiting, looſeneſs, together with a ſudden weakneſs, and a fever, are ſigns of the plague, and more ſo without

a fever

a fever; but if pain be felt in the common emunctories, the plague is not to be concluded from suspected signs.

A reddish pustule, half as big as a vetch, and suppurating, is malignant; for, it soon becomes a livid carbuncle: Carbuncles break out indifferently on all parts of the body, even on the lips, tongue, balls of the eye, *glans* of the *penis*, *&c.* and *bubo*'s on the emunctories only: Hard small glandules about the neck are malignant: *Exanthemata* are always mortal: If *bubo*'s do soon suppurate, there is some hopes of the patient's recovery; and there is no hazard, if such as are not ripe, be opened with a lancet: Several that have been cured by discussing the *bubo*, have afterwards, upon going to infected places, felt a kind of dull pain in that part: The plague in some patients may lie concealed for several days, and afterwards actually break out: If a patient, that begins to recover, be guilty of any great irregularity in diet before the 40th day, a new *bubo* breaks out, and he dies: It is reckoned a very uncommon thing, if a patient, who has been perfectly cured of the plague, should be seised again the same year: If we reduce the disposition of bodies to eight degrees; the disposition of such, as have not been infected, is as 7; and of such, as already have, as 1: A person living for some months in an infected house without being touched, has at length been seised with the plague. Old people, according to *Diemerbroeck cap.* 4. are infected with more difficulty, but young people more easily: Strangers are more exposed to it than the natives: The *Armenians* are of all nations least subject to be infected; they eat but little meat, and live chiefly on onions, leeks, and garlic, and drink wine plentifully: To eat pork in the time of the plague is immediate poison. Nothing is so apt to dispose to the infection, as the passions of the mind, especially excessive grief and sudden fear: Venereal patients are indifferently disposed to the contagion; yet their *bubo*'s suppurating generally degenerate into *fistula*'s: Houses kept clean and neat are not so easily infected as those that are not so: *Cachectic* and *Icteric* patients and others in chronic distempers, do either escape, or get the better of the contagion; whilst others of a blooming complexion, and robust constitution die of it.

Fumigations of juniper-berries, pitch and sulphur are found to be beneficial, if continued day and night, so as to be conveyed from the lower parts thro' the whole house: Perfumes made of vinegar are reckoned to be of some service: Oil of

amber

amber rubbed on the nostrils is very good: A moderate glass of wine, chearfullness, and a regular diet are of considerable service, as preservatives: Blood-letting especially if late, is not adviseable; for, oftentimes it cannot be stopped; and in scarifications the patient has been observed to bleed to death: Gentle vomits are of no service, and strong vomits are very pernicious; purges are present death: The only hope of relief is in sweating and alexipharmics: At *Constantinople* they commonly make use of venice-treacle: *Bezoar*, tho' genuine, is almost of no manner of service: The *Jews* make use of acids: The *Armenians* and *Greeks* of wine and spirit of wine: The *Christians* abstain very religiously for several days from meat and the broth made of it; but the *Turks* are not so scrupulous.

A small tubercle breaking out without any livid spot on several, who have abstained from flesh, such have been abroad and in health for a week, but afterwards eating meat they have died in a very short time: Crude *opium* and all opiates are good as preservatives, and often as curatives: About the quantity of two drachms of oil of *naptha* or white *petroleum*, drank in any vehicle at pleasure, is reckoned a sovereign remedy by the *Turkish* nobility; this agrees with camphire so much celebrated among us: Several patients were said to be cured by drawing blood on the first day of the infection, till they swooned away, and afterwards administring a draught of strong vinegar, in which a drachm of *Sang. Drac.* and *Bol. Armen.* was dissolved: As to blood-letting the Dr. leaves others to determine; only he affirms, that it is a very excellent remedy: A strong friction continued for some hours with bruised garlic, on the breast and back, opposite to the heart, is of service; as is also hellebore-root put transversely on the emunctories into incisions made in the flesh: Chickens or pidgeons, either alive, or cut thro' the middle, and applied to *bubo*'s and carbuncles, and even to the region of the heart and often repeated and not kept on above half an hour, have been found of considerable service. Oil of amber and extract of juniper have been found to succeed in practice: The method prescrib'd by *Diemerbroeck* is in all respects beneficial in this plague, and he has writ very well on the subject, only he derives the cause of it too far back; and *Barbette* is no contemptible author.

In the latter end of *May* 1712. the plague began at *Constantinople*, and in the close of *July* it was at its height; in one day there died upwards of 90; at which time the stated orderly

therly winds blew very violently; and after them there succeeded a pretty strong south wind; the first week after, only 40 died in a day; the second week about 30; and the third under 20, which is the common rate every day at *Constantinople* at this season of the year, even in a good state of salubrity: Thus the plague was suppressed that year in autumn; tho' it is at its height at that time and begins in the middle or latter end of summer; it has likewise been observed, that the plague beginning in spring was suppress'd in autumn: It is observable, that tho' the plague rage exceedingly in *Egypt*, yet it ceases infallibly after the summer solstice; the cause of which is a matter of higher inquiry: This peculiar quality in the climate of *Egypt* does, as to this particular phænomenon of the plague, extend as far as *Smyrna*, the island of *Chio*, and even the streights of the *Hellespont*: It is farther to be observed, that the stated northerly winds, following the course of the sun, convey air, impregnated not only with excrementitious vapours, but likewise nitrous particles: The Dr. had an opportunity of observing this by a kind of garment made of goats hair: This garment seems to be a sort of hygroscope; for, whatever part of it happens to be moistened with the sweat of the hands or neck, and afterwards become dry, tho' wore several months after, yet in half an hour or an hour after sunrise (to wit, when the stated annual winds begin to blow) it will again contract such a dampness, as to be stained with a very deep black spot; and in two or three hours after, the garment dries again, and the spot vanishes. In this there are two things observable; 1. That if the garment be several times moistened in fair water and afterwards dried, this phænomenon of the dampness and spot does not appear, as when it is moistened with sweat. 2. That if it happen to rain at that time, the garment is not so easily moistened and stained, provided it be not actually exposed in the rain: From which the Dr. gathers, that the air conveyed thither from the *Euxine* sea, on the first blowing of the stated winds, is impregnated not only with watery particles, but likewise with nitrous salts; which last are fixed by the congener saline particles communicated by sweat to the garment, and by their union form that stain: Therefore, the air impregnated with these salts may promote the spreading of the pestilential ferment, which probably is of a salino-corrosive nature; and it is not unlikely, that the air of *Egypt*, convey'd thither by the south wind, being very much rarified and divested of such salts, may resist the

the ſpreading of the plague: Add to this, that the warm ſouth wind diſpoſes our bodies to a more plentiful perſpiration.

But if it ſhould be aſked, whether the plague coagulate or greatly diſſolve the blood; the anſwer is, that it does neither primarily and immediately, but that both may happen in proceſs of time, as the active principles of the maſs of blood in different individuals happen to vary: For, the plague primarily and immediately attacks the ſpirits, afterwards the fluids, and produces ſeveral diſorders in the ſolids, according to their various ſtate: Hence according to *Diemerbroeck cap.* 7. there are various ſymptoms in the plague, and thoſe diametrically oppoſite to each other, that cannot be cured by one ſingle remedy, but require the judgment of a ſkillful phyſician, to apply what is proper: Yet generally it is to be obſerved, that the moſt generous remedies ſhould be ſpeedily taken and in large doſes; that at firſt oleoſo-balſamics and volatiles, which are beneficial to the ſpirits, ſhould be adminiſtred; that all evacuations but ſweat ſhould be avoided; and therefore, that no artificial evacuations ſhould be attempted; that the natural ones ſhould be ſtopped by proper remedies; thus a looſeneſs was happily ſuppreſſed by an aſtringent glyſter, in which there was ſome treacle; that if the ſpirits be unruly and their motion exceſſive (which is chiefly known by watchings) they ſhould be laid by opiates; but if on the contrary languid (which is chiefly known by drouſineſs) they ſhould be rouſed by volatiles and preparations of camphire; and that the conſiſtence of the blood may be preſerved entire throughout the whole diſtemper, acids and earthy aſtringents ſhould be made uſe of, to drive the poiſon outwards.

An Account of a luminous appearance in the Air *at* Dublin; *by Mr.* Percival. Phil. Tranſ. N° 364. p. 21.

JANUARY 12. 1719-20 there began an odd appearance in the ſky about 10 o'clock; but it had nothing very remarkable till about ½ an hour after 11, when Mr. *Percival* was called out to ſee it by his ſervants, who had obſerved it about ½ a quarter of an hour and told him it looked juſt like fire: But it firſt appeared to Mr. *Percival* in long ſtreams of light, of a round body, as repreſented at A Fig. 6. Plate II. and very bright; tho' ſome were coloured, as at A *a*; they came before the wind, which was then at weſt, as near as he could conjecture, there not being a cloud in the ſky, and the moon the brighteſt he ever knew.

They had rain about five, but at ſix o'clock the night was clear; the ſtreams of light A A moved very ſlow (there being but

little wind) but as they moved they joined, and ſwelling out in the middle, formed themſelves into the figure *bb* B, continuing to advance ſlowly in that ſhape for about a minute, when the two ends *bb*, approached near each other, as repreſented by the prick'd lines; the advanced part B ran back ſuddenly and with great ſwiftneſs, and joining itſelf with the ends *bb*, form'd itſelf into the figure C, quivering in the upper part, and darting down perpendicularly in ſharp points as at DDD; and its colour changed from a bright light into the colours of the rainbow, but much fainter: It continued thus about a minute, and then the ſharp points DDD, gathering themſelves up into C, it again changed into a ſquare ſheet of light, as at E, and ſwelled out at F, as before at B; and advancing leiſurely exhibited the ſame ſcene as before, till it ſeemed to diſperſe at a great diſtance into ſmall thin light clouds; tho' it is probable, that to thoſe who ſaw it in a like ſituation, as it went along, it might make the ſame appearance as it did to Mr. *Percival*: He was very particular in obſerving it, and he delineated it very exactly, as repreſented in the Fig. having obſerved it upwards of ¼ of an hour: The beginning of it was very like the *aurora borealis*, which was very frequent this winter in *Ireland*.

Of the infinity of the Sphere of fixed Stars; *by Dr.* Halley. Phil. Tranſ. N° 364. p. 22.

THE ſyſtem of the world, as it is now underſtood, is taken to poſſeſs the whole abyſs of ſpace, and to be as ſuch actually infinite; and the appearance of the ſphere of fixed ſtars, ſtill diſcovering ſmaller and ſmaller ones, as you apply better teleſcopes, ſeems to confirm this doctrine; and indeed, were the whole ſyſtem finite, it, tho' never ſo extended, would ſtill poſſeſs no part of the *infinitum* of ſpace, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded on all ſides with an infinite *inane*, and the ſuperficial ſtars would gravitate towards thoſe near the center, and with an accelerated motion run into them, and in proceſs of time coaleſce and unite with them into one; and ſuppoſing a ſufficient ſpace of time, this would be a neceſſary conſequence: But if the whole be infinite, all its parts would be nearly *in æquilibrio*, and conſequently each fixed ſtar, being drawn by contrary powers, would keep its place, or move till ſuch time, as from ſuch an *æquilibrium* it found its reſting place; on which account ſome, perhaps, may think the infinity of the ſphere of fixed ſtars no very precarious poſtulate.

But

But to this there are two objections, which are rather of a metaphysical than physical nature; and first this supposes, as its consequence, that the number of fixed stars is not only indefinite, but actually more than any finite number; which seems absurd *in terminis*, all number being composed of units, and no two points or centers being at a distance more than finite: But to this it may be answered, that by the same argument we may conclude against the possibility of eternal duration, because no number of days, or years, or ages, can compleat it.

Another objection urged against it is, that if the number of fixed stars were more than finite, the whole superficies of their apparent sphere would be luminous; for, that those shining bodies would be more in number than there are seconds of a degree in the area of the whole spherical surface, which the Dr. thinks cannot be denied: But if we suppose all the fixed stars to be as far from each other, as the nearest of them is from the sun; that is, if we may suppose the sun to be one of them, at a greater distance their disks and light will be diminished in the ratio of squares, and the space to contain them will be increased in the same ratio; so that in each spherical surface the number of stars it might contain will be as the square of their distances: Suppose the distances immensely great, as we are well assured they must be; and thence by an obvious calculation it will be found, that as the light of the fixed stars diminishes, the intervals between them decrease in a less proportion; the one being as the distances, and the other as the squares thereof reciprocally: Add to this, that the more remote stars, and those far short of the remotest, vanish even in the nicest telescopes, by reason of their exceeding minuteness: So that, tho' it were true, that some such stars are in such a place; yet their beams, aided by any help hitherto known, are not sufficient to strike our sense; after the same manner as a small telescopic star is by no means perceivable by the naked eye.

Of the Number, Order and Light of the fixed Stars; *by the Same.* Phil. Transf. N° 364. p. 24.

DR. *Halley*, having in the preceeding *Transaction* proposed some arguments, that seemed to him to evince the infinity of the sphere of fixed stars, as possessing the whole abyss of space, or the *το παν*, which at present is generally understood to be necessarily infinite, thence laid down what may seem a very metaphysical paradox, *viz.* that the number of fixed stars must consequently be more than any finite number, and some of them more

more than at a finite distance from others. This seems to involve a contradiction; but it is not the only one that occurs to those who have undertaken freely to consider the nature of infinite, which, perhaps, the very narrow limits of human capacity cannot attain to.

The Dr. having attentively examined what might be the consequence of an hypothesis, that the sun being one of the fixed stars, all the rest were as far distant from each other, as they are from us, by a due calculation finds, that there cannot, upon that supposition, be more than 13 points in the surface of a sphere, as far distant from the center of it, as they are from each other; and he believes it would be hard to find how to place 13 globes of equal magnitude, so as to touch one in the center: For, the 12 angles of the *icosaedron* are very little more distant from each other than from its center; that is, the side of the triangular base of that solid is very little more than the semidiameter of the circumscribed sphere, it being to it as 21 to 20 nearly: So that it is plain, that somewhat more than 12 equal spheres may be posited about a middle one; but the spherical angles or inclinations of the planes of these figures being incommensurable with the 360 degrees of the circle, there will be several interstices left between some of the 12, but not such as to receive in any part the thirteenth sphere.

Hence it is no very improbable conjecture, that the number of the fixed stars of the first magnitude is so small, because this superior appearance of light arises from their nearness; those that are less, shewing themselves so small by reason of their greater distance: Now there are in all but 16 fixed stars, that can indisputably be accounted of the first magnitude; whereof four are without the zodiac, *viz. Capella*, *Arcturus*, *Lucida Lyræ* and *Lucida Aquilæ* to the north; four in the way of the moon and planets, to wit, *Palilicium*, *Cor Leonis*, *Spica* and *Cor Scorpii*; and five to the southward, that are seen in *England*, *viz.* the *foot* and *right shoulder* of *Orion*, *Sirius*, *Procyon* and *Fomahant*; and there are three more that never rise in our horizon, viz. *Canopus*, *Acharnâr* and the *foot* of the *Centaur*. But that they exceed the number 13 may easily be accounted for from the different magnitudes observable in the stars themselves; and perhaps some of them may be much nearer to each other than they are to us; this excess of number being found singly in the signs of *Gemini* and *Cancer*; and indeed within 45 degrees of longitude or $\frac{1}{8}$ of the whole, there are no less than 5 of these 16 to be seen: If therefore, their number be supposed 13, omitting

niceties

niceties in a matter of ſuch irregularity; at twice the diſtance from the ſun there may be placed four times as many, or 52; which with the ſame allowance, would nearly repreſent the number of the ſtars, we find of the ſecond magnitude: So 9 × 13 or 117 for thoſe at three times the diſtance; and at 10 times the diſtance 100 × 13 or 1300 ſtars; which diſtance may perhaps diminiſh the light of any of the ſtars of the firſt magnitude to that of the ſixth; it being but the hundredth part of what at their preſent diſtance they appear with. But if, ſince we have room enough for it, we ſhould ſuppoſe the ſphere continued to 10 times the laſt, or 100 times the firſt diſtance, the number of ſtars would be 130,000, and they would appear but with the 10,000th part of the light of a ſtar of the firſt magnitude, as we now ſee it: This is ſo ſmall a pulſe of light, that it may well be queſtioned, whether the eye, aſſiſted with any artificial help, can be made ſenſible thereof: But 100 times the diſtance of a ſtar we ſee, is ſtill finite: Whence the Dr. leaves it to thoſe who conſider it attentively, to draw the concluſion.

An Account of the Method of making Sugar from the Juice of the Maple-tree *in* New England. *by Mr.* Dudley. Phil. Tranſ. N° 364. p. 27.

MAple-ſugar is made of the juice of upland maple, or maple-trees that grow upon the highlands: You box the tree as it is call'd, *i. e.* make a hole with an ax or chiſſel into the ſide of the tree, within a foot of the ground; the box you make ſhould hold about a pint; and therefore it muſt ſhelve inwards, or towards the bottom of the tree; you muſt likewiſe bark the tree above the box, to direct the juice to the box.

You muſt alſo tap the tree with a ſmall gimblet below your box, ſo as to draw the liquor off: When you have pierced or tapp'd your tree or box, you put in a reed or pipe, or a bit of cedar cut into a channel; and put a bowl, tray or ſmall caſk at the foot of the tree to receive your liquor, and ſo tend the veſſels as they are full.

After you have got your liquor, you boil it in a pot, kettle or copper: Ten gallons will make ſomewhat better than a pound of ſugar.

It becomes ſugar by the thin part evaporating in boiling; for, it muſt be boil'd, till it is as thick as treacle: Ten gallons muſt be boiled to a pint and a half.

A kettle of 20 gallons will be near 16 hours in boiling, before

before you can reduce it to three pints; a good fire may do it sooner.

When you take it off, you must keep continually stirring it, in order to make it sugar; otherways it will candy as hard as a rock.

Some put in a little beef-sewet, as big as a walnut, when they take it off the fire, to make it turn the better to sugar, and to prevent its candying; but it will do without. A good large tree will yield 20 gallons: The season of the year for it, is from the beginning of *February* to the beginning of *April*.

Mr. *Dudley* adds, that their physicians look upon it not only to be as good for common use as the *West India* sugar, but to exceed all others for its medicinal virtue.

An Account of a Boy who liv'd a considerable Time without Food; by Dr. Blair. Phil. Transf. N° 364. p. 28.

FEB. 3. 1716. One *Gilbert Jackson* in *Carse-grange* in *Scotland*, about 15 years of age, fell sick and complained of pains all over his body; and towards the latter end of the said month, he was siez'd with a violent fever, in which he continued for three weeks and then recover'd.

The beginning of *April* following he relapsed again, his fever continuing for three weeks; and during that fever he had a shaking in his body as if he had been paralytic.

On the 10. of *June* following he relapsed a third time, when he became dumb, lost his appetite entirely, and the use of his limbs, and continued without either eating or drinking, tho' all means were us'd to make him do both; but he recover'd of his fever on *May* 17. 1717, yet he still continued dumb, and without either eating or drinking, or having the use of his limbs, till the 10. of *June* the said year; when he was again siez'd with an extraordinary fever, and the next day he recover'd his speech; but continued in the fever, without either eating or drinking, or having the use of any of his limbs till the 11. of *November* following, when he recover'd his health pretty well and the strength of one of his legs; and thus he continued without either eating or drinking, only washing his mouth sometimes with water; and when he saw the rest of the family go to meals it caused great uneasiness in him, and he always retir'd.

On the 10. of *June* 1718 he had a fever again, which continued till the beginning of *September* after, when he recover'd of

of the ſaid fever, tho' he never could be prevail'd on to take any kind of meat or drink; and thus he continued in pretty good health and freſh colour'd till the 9th of *June*, 1719, when he was again taken with a ſevere fever; and on the 10th at night the patient's father preſs'd him extremely to take a little milk boil'd with oatmeal, which at length he agreed to; and he took a ſpoonful of it, which ſtuck ſo long in his throat, that his parents thought he had been choak'd; and ever after he took a little food; but ſo very little, that a half-penny loaf ſerv'd him eight days: All the time he faſted, he never had any evacuation by ſtool or urine; and it was 14 or 15 days after he began to eat, that he had any; he continued in pretty good health; but wanted the uſe of one of his limbs.

This extraordinary caſe is confirm'd by the affidavit of the parents at the court of the barony of *Errol*, holden at *Errol*, *Dec.* 26. 1719, by Mr. *Charles Brown*, baily to the Right Honourable the Earl of *Northeſk*, *Gilbert Anthone* clerk, and *Charles Gill* officer.

A Method of diſcovering the Virtues of Plants *by their external Structure; by the ſame.* Phil. Tranſ. N° 364. p. 30.

THE ancients, without any of thoſe aids made uſe of by the moderns, have handed down to us ſuch an account of the virtues of plants, which are more particularly uſeful in phyſick, that all the laborious endeavours of their inquiſitive ſucceſſors have never been able to outdo them.

It muſt have been a long tract of experience, that enabled *Dioſcorides* and *Theophraſtus* to collect ſuch a laſting catalogue of the virtues of plants, as ſcarce any thing has been added to it even to this day. The Royal Academy at *Paris* have been at great pains to find out the virtues of plants by chemical analyſis, and ſeveral other experiments; of which we have the abſtracts in *Tournefort*'s *Hiſtoire des plantes aux environs de Paris*, and *Tauvry*'s *Traité des medicaments*: But theſe laborious endeavours only ſerve to confirm what the ancients have advanced, without making any new diſcovery: For, *Tournefort*, after having made the experiments with the *tourneſol* and blue paper, and given an exact account of the ſeveral active chemical principles obſerv'd in different plants, uſually concludes, *ainſi il n'eſt pas ſurprenant s'il a de telles vertues*; therefore, it is not ſurpriſing, if it is endu'd

with ſuch virtues; which is nothing but giving a reaſon, why the ancients believ'd they were good for ſuch a diſtemper.

The means us'd by our anceſtors to diſcover the virtues of plants and their uſe in the ſeveral diſeaſes, as they were the moſt ſimple, ſo they are the moſt beneficial at this very time: It ſeems they narrowly conſider'd their external appearance, and concluded, if ſuch a plant partake of ſuch virtues, ſuch another plant ſo very like it, muſt be endu'd with the ſame *ex. gr. Apium* and *Fœniculum* have the ſame manner of flowering; both produce their ſeed after the ſame mannner; their roots are both alike, being long, white, ſtreight, carnious, *&c.* Therefore, ſince a long tract of experience, handed down by tradition, ſhews, that ſuch a plant has ſuch virtues, ſuch another like it muſt have the ſame. Thus we find *Apium*, *fœniculum*, *Petroſelinum* all join'd together and preſcrib'd as opening roots in the Diſpenſatory.

This induced that expert Botaniſt, and diligent inquirer into the *Materia Medica*, Dr. *Herman*, to lay down theſe general maxims, *Quæcunque flore et ſemine conveniunt eaſdem poſſident virtutes*: And, *omnia ſemina ſtriata ſunt carminativa.*

Mr. *Petiver* obſerves, that the *plantæ umbelliferæ*, *galeatæ*, *verticillatæ*, *tetrapetalæ*, *ſiliquoſæ* and *ſiliculoſæ*, for the moſt part, have the ſame virtue and uſe; and in a letter to the Dr. he obſerves, that the *plantæ flore ſtamineo*, which he calls blink-flowers, as hops, nettles, docks, ſorrels, beets, blites, ſpinage, oraches, *bonus Henricus* or *Engliſh* mercury, and *Kali minus album*, are all good ſallads, raw or boil'd; as alſo the *leguminoſæ*, or pea-kind, as peaſe, beans, *phaſeoli* are good nouriſhing food for men; and tares, trefoils, *medicæ*, *loti* and ſaintfoins good fodder for cattle: To theſe he adds the *frumentaceæ* or *cereales*; as wheat, rye, and oats in *Europe*; and the maiz, millet, panick and ſorgum in the *Indies*, make good bread; and that from barley and rice we have good fermented and ſpirituous liquors. To theſe he adds, that the *iris* or flag-kind in foreign parts afford us drugs of no mean virtue and uſe; as ginger, galingal, turmeric, zedoary, caſſumuniar and cardamums: The *laurus* or bay-kind has ſome noble attendants of the ſame tribe with itſelf; as *cinnamon*, *caſſia lignea*, *malabathrum*, *folium Indicum*, and the *camphire* tree.

In an anſwer to this the Dr. added, that all the *pappeſcentes* and *lacteſcentes*, as the *ſonchus*, *dens leonis*, *hierachium*, *lactuca*

uca, *cichoreum*, *endivia*, *tragopogon* and *ſcorzonera* have the ſame virtues, and ſerve for the ſame uſes both in the kitchen and ſhops: All the *aſperifoliæ*, as *borago* and *buglоſſum* are call'd coolers in a more or leſs intenſe degree; for, ſome are aſtringent, as *conſolida*; others narcotic, as *cynogloſſum*. All the *galeatæ* and *labiatæ* for the moſt part conſiſt of ſubtile particles, and are therefore, cephalics, as *lavendula*, *roſmarinus*, *majorana*, &c. *mentha*, *pulegium*, *meliſſa* are hyſterics; *Salvia*, *horminum*, &c. have an attenuating virtue: And a fourth ſort, as *bugula*, *lamium*, &c. are ſomewhat aſtringent; ſo that by having an idea of the virtues of a *majorana*, *mentha*, *ſalvia*, *lamium*, we come to know the virtues of all of the ſame tribe. All the *papavera* are narcotic: The *eſulæ* and *tithymali* are cathartic; tho' both theſe are *lacteſcentes*, yet they differ from the *pappeſcentes*: All the *malvæ* are chiefly emollient: The pentaphyllous kind are aſtringent; as alſo the plantains; the corymbiferous kind, are either ſtomachics, hyſterics and vermifuges: The gentian, bitters, ſtomachics, hyſterics, febrifuges: The *pomiferæ ſcandentes*, as cucumbers, melons, &c. are coolers; but ſome are cathartic, as *cucumis ſylveſtris* and *colocynthis*. The *convolvuli*, as *mechoacanna*, &c. are purgative; to which *jalappa*, both in flower and fruit, is near of kin: *Digitalis* and *gratiola* are emetic and purgative: The ſquamous and bulbous roots are emollient, and more or leſs acrid: Thus *allium*, *cepa*, *porrum*, unboil'd, are hot, diuretic and lithontriptic: All the *ſeda* are coolers.

Thus at the firſt view, without knowing the characteriſtics or peculiar marks ſo nicely as Botaniſts do; but only exactly obſerving the external figure of the plant, when the virtue of one ſpecies is known, the virtues of all of the ſame tribe may be gueſs'd at, if not fully determined.

The next ſimple method of the ancients to diſcover the virtues of plants ſeems to have been the taſte and ſmell: Thus *apium* and *petroſelinum* have a like taſte; therefore, they are to be preſcrib'd together: The ſeeds of *fœniculum* and *aniſum* have much the ſame taſte and ſmell; and therefore, both of them muſt be carminative or expellers of wind, &c. The ancients had likewiſe recourſe to the temperament and qualities; ſuch as hot and dry, cold and moiſt, in the 1, 2, 3 and 4th degrees: But ſince the taſte is not always the ſame in one perſon, and that different perſons have different ſenſations; this, as being too much ſubjected to the different tempers and imaginations of people, is deſervedly exploded.

The Dr. compos'd a compendious ſcheme of all the plants us'd in phyſick; in which, that it might be leſs liable to objection, and that he might not ſeem to introduce any innovation in the diſtribution, he has not ſo ſtrictly obſerv'd the making their peculiar marks and virtues agree, as the diſtributing them according to their operations.

The firſt diſtribution is, by joining together all thoſe that are preſcrib'd under one title in the ſhops; as the opening roots, emollient and capillary herbs, cordial flowers, hot and cold; and the greater and leſſer ſeeds: In this the Dr. has not kept to the Diſpenſatory catalogue; but added ſeveral of the ſame tribe, that he might give a ſpecimen of what is propos'd concerning their virtues and characteriſtics: Thus, he has added *cuminum* and *meum* to *fœniculum*; *laurus Alexandrina* and *hippoglоſſum* to *ruſcus*; *alcea* to *malva* and *althea*: *bonus Henricus*, *atriplex*, &c. to *beta*, under the title of *oleraceous emollients*; *lingua cervina*, *polypodium*, &c. to the capillary herbs; and ſo on in the cordial flowers, and in the hot and cold ſeeds.

The Dr. has in the ſecond place diſtributed the plants into ſuch as are alterants and evacuants: The alterants are divided into ſuch as conſiſt of groſs, and into ſuch as are ſaid to conſiſt of ſubtile particles: Thoſe conſiſting of groſs particles are aſtringent, preventing abortion and ruptures, ſtopping the immoderate flux of the *menſes*, the *fluor albus*, *diarrhœa*, *dyſentery*; and are good in burnings, bruiſes, cancers, ſpitting of blood: Groſs medicines are narcotics, vulneraries, good for ſcrophulous tumours, ſquinancy, and are coolers.

Plants conſiſting of ſubtile particles are aperient; ſuch are all opthamics, arthritics, nephritics, lithontriptics, diuretics and hydropics: They are alſo pectoral, anti-apoplectic, paralytic, hyſteric, hypocondriac, promoters of the birth, febrifuges, ſcorbutics, ſtomachics, vermifuges.

The evacuating medicines are emetic, or ſuch as work upwards; or laxative and purgative, ſuch as work downwards: The nutritive medicines are the *plantæ cereales* and *leguminoſæ*.

Here it to be obſerv'd, that the Dr. has not inſerted any plant in this table, but ſuch as are natives of *Britain*, or ſuch as are cultivated in *Britiſh* gardens; and to render it ſtill the more uſeful, he has added ſuch particular parts, as are us'd in the ſhops, *viz.* the root, herbs, leaves, tops, flowers, fruit, nuts, bark and wood.

Having thus reduced within a ſmall compaſs the moſt conſiderable virtues of plants, both general and ſpecific; and ſhewn the

he most easy, simple, and natural method of discovering them, he Dr. would not be so far misunderstood, as if he were averse rom using other experiments to find them out; on the contrary, e could heartily recommend another method, hitherto much eglected, and which he is persuaded would be of considerable se, if accurately gone about; and that is their infusion in diffe- ent liquors, in order to find out the proper *menstruum* for ex- racting their more useful parts.

Every physician is sensible, that there are several simples and hese specific too, which exhibited in substance, are of great fficacy; whereas if their contexture be dissolv'd, their parts can ever be so re-united, as to produce the same effect: Thus ortex *Peruvianus* is never so effectual, as when administred in owder: That there are others which will communicate their seful particles, when infus'd, to one liquor and not to another; nd that the same substance will impregnate two liquors diffe- ently, according to the different *menstruums*. That expert hemist M. *Lemery* advises to infuse *opium* in water and spirits f wine separately; and afterwards to mix both infusions toge- her, in order to make the *laudanum* or extract; very well con- idering, that the water will be impregnated by the more soluble aline particles, whereas the spirits will only imbibe the more esinous; for, water is the proper *menstruum* for a saline substance, which will not dissolve in spirits of wine; this rather hardening nd preserving it from being dissolv'd, either by air or water: Thus the most convenient way to preserve the volatile salts of animals, is to keep them in brandy; and every one knows, hat water immediately dissolves sugar, which brandy will not do: Therefore, *senna* will communicate its purgative quality to water or ale, having its saline particles more disengaged; but the purgative quality of *jallop* consisting in its rasin, requires wine or brandy for the *menstruum* or dissolvent.

Therefore, Dr. *Blair* supposes, that having recourse to the proper *menstruums*, is a most proper means of finding out the virtues of plants: A simple may be infus'd in rain-water, snow-water or pure fountain-water; if its texture be loose, and it abound in saline particles, those pure elements will be im- pregnated thereby; but if the texture be more compact, firm and solid, and if its particles be more fix'd, it must be infus'd in mineral waters; or by the addition of a proportional quan- tity of the fix'd salt of a plant, a proper *menstruum* may be prepar'd: And next to the exhibiting of bitters in substance, as wormwood, gentian, and camomile flowers, this is the most

most convenient way of administering them; not but that their tincture extracted by brandy or wine may do very well: But since they greatly abound in a fix'd salt, a great deal of their virtue may be communicated to a less spirituous liquor, when a more spirituous will not extract it: The proper means to know which *menstruum* will best extract the more useful parts of any simple, or rather suspend its more solid particles, is to use the hydrostatical balance; when weighing the *menstruum* before infusion, and after the matter has been infus'd for some time, it will soon be observ'd by the increase of the weight, how far the *menstruum* is impregnated, and which is the most proper dissolvent: The properest method of exhibiting the fix'd simples, if not in substance, is by decoction, infusion or tincture (*N. B.* It is call'd infusion, when the *menstruum* is either water, wine or ale, but a tincture when brandy is made use of) and the best way to obtain the useful particles of volatile or subtile substances is by distillation. These may, it is true, be proper ingredients for an infusion or tincture: But there are a great many fix'd substances as improper for distillation, as the volatile are improper for extracts. Thus the Dr. has shewn the means of discovering the virtues of plants, without dissolving their texture: But if any have a mind to do it by chemical analysis, this is not to dissuade them.

A Case in Surgery that is commonly mistaken for a Fracture of the Patella; *by Mr.* Deverel. Phil. Trans. N° 365. p. 44.

ONE *Richard Burt* was thrown from his horse, and in the fall receiv'd such a hurt in one of his knees, as made him uncapable of remounting: He felt somewhat crack in that knee (as he express'd it) before it touch'd the ground. Upon examining the part, Mr. *Deverel* found (as he then thought) the ends of the fractured bone drawn upwards of four fingers distance from each other: But upon a stricter examination of the parts, he found the *patella* (which was drawn upwards by the extensors of the leg) retain'd its natural figure, and that the hardness which was felt below was the end of the torn ligament that ties it to the *tibia*: The ends of the ligament were brought as near as possible, and kept so about three weeks without any remarkable accident: He then began to walk, which was a little too soon, causing thereby some pains, and loosening the cicatrice, which made it the longer before it was perfectly firm; however he afterwards walk'd without

any

any perceivable lameneſs. Mr. *Deverel* met with two others in the ſame caſe, one of whom did not walk ſo well as ſhe uſed; tho' all imaginable care was taken: For, it is hardly to be expected, that one in ten, to whom this accident happens, ſhould ever go right; it being next to impoſſible, that the ends of the torn ligament ſhould be ſo exactly placed and retained, as not to lie over each other.

Parée in *ch.* 22. book 15 affirms, that he never ſaw any who had this bone fractured, that did not halt ever after: To this Mr. *Deverell* agrees, but he differs from him in his notions of the caſe, and the cauſe of the lameneſs. *Hildanus* in his *Obſ. Chirurg. Cent.* 5. *Obſ.* 88. *p. m.* 485. has given us an account of a tranſverſe fracture of this bone, which, after all the ſymptoms were removed, was cured; 'but, ſays he, a lameneſs and 'a great weakneſs of the whole leg enſued; ſo that the patient could not walk but with the utmoſt difficulty.' He, afterwards mentioning the cauſe of the lameneſs, queries, whether what *Parée* ſays in his *Exc. lib. de Oſſibus lib.* 4. *cap.* 2. can be the cauſe of the lameneſs; his words are to the following purpoſe; 'but tho' I ſhall not attempt to call in queſtion 'the juſtneſs of this opinion, yet there remains one difficulty, 'and that is, whether when the *patella* is fractured, the protuberance of the *callus* can be ſuch, as to fill up this cavity '(which is very conſiderable between the *femur* and *Os tibiæ*) 'ſo as to hinder the motion and action of the knee? For, we 'obſerve for the moſt part in other fractures of bones (unleſs 'there happen a very great contuſion of the bone and *perioſteum*) that nature does ſo curiouſly knit the bones together, 'that there is ſeldom any veſtige of the fracture: For, the '*perioſteum*, while entire, hinders the matter of the *callus* 'from becoming an excreſcence, unleſs, *&c.* Farther in the 'caſe of the preſent patient it will appear by what follows, 'that the *callus* was not the cauſe of the lameneſs; wherefore, we muſt here make a diſtinction.' After this he goes on to ſhew, how many different ways this bone may be fractured, but he does not obſerve, that he ever ſaw it fractured any other way (unleſs in a gun-ſhot-wound) than tranſverſely.

Ruyſch in his *Centuria Obſ. Anatomico-Chirurg.* 4. *Obſ.* 3. writes to the following purpoſe; 'ſome obſtinately affirm, 'that the *patella* cannot be fractured by reaſon of its hardneſs; 'but I have found it fractured not only by a great fall, but 'without any; a pretty robuſt man coming down from the 'end of a bridge had almoſt fallen to the ground by one foot ſlipping,

'slipping, but recovering himself he did not come to the 'ground; however in the struggle his *patella* was fractured 'transversely, and that so plainly, that one's hand could go 'between the fracture; for one part might be felt above, the 'other below the knee.'

This observation is in all its circumstances so like that mentioned above by Mr. *Deverel*, especially, in that the fracture was made without a fall, that he is very apt to believe it is the very same; and Mr. *Deverel* supposes *Ruysch* might be deceived by mistaking the hard extremity of the ligament for the fractured bone.

And when these sorts of accidents, which are now called fractures of the *patella*, come to be nicely examined; they will perhaps be found to be generally of the same nature.

An Account of the Antiquity of the Venereal Disease; *by* Mr. Becket. Phil. Transf. N° 365. p. 47.

MR. *Becket*, before he comes to prove, that the venereal disease, when it came to be confirmed, was frequently known in *England* some hundreds of years before the siege of *Naples*, endeavours to refute the opinion of such, who take it to have had its rise there. It is true, there have not been wanting several modern authors who have asserted it; but Mr. *Becket* makes it appear to be an error as inconsiderately and hastily received, as chimerically started by some author; who, because several writers about that time, observing the disease to begin in the *pudenda*, distinguished it from another, with which it was before confounded, must likewise assert its being a new distemper, and assign a certain time and place for its rise: Now one might with all the reason in the world expect, that if the disease had its original there, it must have been so certainly and infallibly known, that there could have been no uncertain opinions about it; but that the physicians who resided in or near the place, and those more especially who interested themselves so far as to write of it, must have all of them to a man agreed upon the certainty of a thing, the truth of which was so easily attainable. But on the contrary, *Nicolas Leonicenus*, the first *Italian* physician that wrote of this disease, and who lived at the very time, when *Naples* was besieged, is so far from acknowledging it to have had its rise there, from the *French* soldiers conversation with the *Italian* women, that he does not allow it to be the consequence of impure embraces, so little did he know of its true cause. It

was

vas likewise about this time that Pope *Alexander* VI. engaged *Jasper Torella* to write of this distemper; who is so far from llowing it to have had its original there, that he tells us, the strologers were of opinion, it proceeded from he knows not vhat particular constellations: Neither does *Sebastianus Aquianus*, who liv'd at that time, or *Antonius Scanarolius*, who vrote in 1498, about four or five years after the siege, allow it o be any other than an ancient disease: Nor do several other uthors, then living, mention one word of this *Neapolitan* tory. But it seems *Ulricus de Hutten* a *German* knight, who vas no physician, positively affirms this disease to have had its ise there; but how he should come to know this, who lived t such a distance from the place; and how they, who were hysicians, residing, as it were, on the spot, should be ignoant of it, will be as much credited, as his following inconsisent relation. He tells us this disease was unknown till 1493, r thereabouts; that he himself had it when a child; and onsequently, that it was hereditary, or from the nurse. He vrote his book of this distemper at *Mentz*, where it was rinted by *John Scheffer* in *quarto* in 1519. Now if we allow im to be but 27 years of age, when he wrote (for, he cannot e supposed to be less, who before this time took upon him to ure his father of the venereal disease, without the assistance f any physician or surgeon) he must have had the distemper pon him, according to his own account, before ever it was in eing.

In *Phil. Trans.* N° 357. Mr. *Becket* thinks he has sufficiently roved, that the first degree of the venereal disease was very ommon here in *England*, some hundreds of years before it is ommonly said to have been known in *Europe*; consequently, here will be no reason to suppose we were strangers to it, vhen it came to be confirmed abroad: Now when it was in his confirmed state, the writers of those early times looked pon it as an entirely new disease, and not a consequence of ny disorder before contracted; because they were not appris'd, hat, the first symptoms being removed, and the disease in ppearance cured, it should afterwards discover itself in such manner, as should not seem to have the least analogy with he symptoms, that first attacked a part, which had for a coniderable time been free from any misfortune. But because he symptoms are the only true characteristics, whereby we an infallibly distinguish one disease from another: Mr. *Becket* roduces sufficient authorities to shew, that the symptoms were

all of them known and deſcribed by ancient phyſical and chirurgical writers, juſt as they appear in the venereal diſeaſe at this day.

Mr. *Becket* thinks he has in *Phil. Tranſ.* N° 357. made it ſufficiently appear, that the firſt degree of this diſeaſe was anciently known in *England* by the name of the *brenning* or *burning*; and that it was the ſame thing with what we now call a *clap*. The ſymptoms, which are uſually its concomitants, are the *phymoſis* and *paraphymoſis*, both which are accurately deſcribed, with proper remedies for them, by Mr. *John Arden* in a manuſcript of his, curiouſly written on *vellum* and beautifully illuminated. The imprudent method of cure of this firſt degree of the venereal malady is ſometimes attended with a caruncle in the *urethra*, which was anciently a diſeaſe very common in *England*: For, not to mention other early writers, the above-mentioned author gives us the caſe of a certain Rector, that had a ſubſtance, reſembling a wart, growing in the *penis*; which in another place he ſays frequently happens; and of another, who had ſuch an excreſcence, as big as a ſmall ſtrawberry, which, ſays he, proceeded from the purulent matter that remained in the *urethra*.

And indeed, there is not any ſymptom of the venereal diſeaſe Mr. *Becket* finds ſo often mentioned, as this of the caruncle; inſomuch that it ſeems to have been more common in thoſe early times than at this day: But this muſt be certainly owing to the ſmooth and oily remedies they were continually injecting, which by their relaxing and ſoftening the fibres of the part, muſt neceſſarily diſpoſe the contexture of the ſmall blood-veſſels, lodged at the bottom of the little ulcerations, to fill with nutritious juices and to extend in ſuch manner, as to form ſuch fungous excreſcencies; and ſo ſolicitous were they to remove theſe inconveniencies, that they made uſe of ſeveral ways by corroſives and other methods to accompliſh this end; and a very early writer among us has given a very methodical and curious tract on this ſubject, wherein he recommends the removing them by the medicated candle, which we uſe at this day; and lays down divers other inſtructions in relation thereto; which, probably, makes it the beſt diſcourſe on this ſubject that was ever yet written. The ſame author takes notice of thoſe obſtinate ulcers, that happen on the *glans* and the neighbouring parts, now called ſhankers; and the great trouble our ancient authors found in attempting their cure ſufficiently ſhew them to have had their original from a venereal infection.

Our early authors are very full in their accounts of theſe ſeve-
l ſymptoms of the venereal diſeaſe, and of others, when the
iſeaſe was in a more confirmed ſtate, to which they appropri-
ed particular names, perhaps more ſignificant and expreſſive
an thoſe impoſed by modern authors: Thus for inſtance, the
ubo's in the groin they called *dorſers*, for which a reaſon was
ſſigned *Phil. Tranſ.* N° 357; and the venereal nodes on the
hin-bones they called the *boonhaw*, which gives us a perfect
dea, not only of the part affected, but after what manner it was
iſeaſed; for, the old *Engliſh* word *hawe* ſignified a ſwelling
f any part: Thus for inſtance, a little ſwelling on the *cornea*,
as anciently called the *hawe* in the eye, and the ſwelling that
equently happens on the finger on one ſide the nail, was called
e white *hawe*, and afterwards *whitflaw* or whitlow. The
roceſs the laſt mentioned author recommends for curing the *boon*
r *bone-hawe* is to uſe a plaiſter, which had a hole cut in the
iddle to circumſcribe it; and applying a cauſtic of unſlacked
me and black ſoap incorporated together, which, with the
laiſter and bandage, was to be faſtened on the part four hours
r longer; after this he proceeds to ſeparate the ſlough, &c.
his practice of his ſeems to have been found out by accident.
or, he tells us, when he was a young practitioner, having ap-
lied both the natural and artificial arſenic to a patient's leg, it
ortified the fleſh in a ſurpriſing manner; but the eſcar coming
ff by proper digeſtives, and leaving the bone bare, he ſcraped
with an inſtrument for ſeveral days, and dreſſed it with incar-
atives; but this proving unſucceſsful, he continued to ſcrape it
ll he obſerved it to move under the inſtrument; after which
aving ſeparated it, he found the ſore covered with new fleſh,
nd that the bone was four inches in length, two in breadth and
ery thick; upon the removal of which the patient was ſoon
ured.

Thus it is probable, that this obſervation of this great man
d our predeceſſors to practiſe the very ſame method; and we
o at this day in our hoſpitals treat the venereal nodes on the
ins exactly as is here deſcribed; where we obſerve the ſame
ppearances, he ſo long ago took notice of; and it is not in the
aſt to be doubted, but the *boonhaw* and our venereal nodes are
e ſame diſeaſe. By the appearance of ſome of the laſt of the
bove-mentioned ſymptoms, we infallibly judge the patient has
ad the infection upon him a conſiderable time, and that the di-
aſe is making its gradual advances, to the corrupting and de-
roying the whole frame of the body: That this was the concluſion

of the miseries of those, who gave themselves up to the embraces of lewd women, in those early times as well as now, is evident from some remarkable instances in a M.S. (in *Lincoln College Oxon.*) writ and collected by one Mr. *Tho. Gascoigne* a Dr. of divinity, whose words are '*Novi enim ego Magister Thomas Gascoigne, licet indignus, sacræ theologiæ Doctor, qui hæc scripsi & collegi, diversos viros, qui mortui fuerunt ex putrefactione membrorum suorum genitalium & corporis sui; quæ corruptio & putrefactio, ut ipsi dixerunt, causata fuit per exercitium copulæ carnalis cum Mulieribus. Magnus enim Dux in Anglia, Scil. J. de Gaunt, mortuus est ex tali putrefactione membrorum genitalium & corporis sui, causata per frequentationem mulierum. Magnus enim fornicator fuit, ut in toto regno Angliæ divulgabatur, & ante mortem suam jacens sic infirmus in lecto, eandem putrefactionem Regi Angliæ Ricardo secundo ostendit, cum idem Rex eundem Ducem in sua infirmitate visitavit, & dixit mihi, qui ista novit unus fidelis sacræ Theologiæ Baccalaureus. Willus etiam longe vir maturæ ætatis, & de Civitat. Lond. mortuus est ex tali putrefactione membrorum suorum genitalium & corporis sui, causata per copulam carnalem cum mulieribus, ut ipsemet pluries confessus est ante mortem suam, quum manu suâ propriâ eleemosynas distribuit, ut ego novi, An. Dni.* 1430.'

Now that those instances from *Arden*, and those from *Gascoigne*, who was then Chancellor of *Oxford*, could possibly be no other than venereal cases, is plain: Certain it is, that no other disease is got by carnal conversation with women, which first attacks the genitals, causing a corruption and putrifaction of them, and afterwards of the whole frame of the body, but that which is venereal: For, nothing is more commonly known at this day than that after venereal embraces with an impure woman, the *penis* is the part where the scene is first laid for the succeeding tragical appearances; and there and in the neighbouring parts, do the symptoms of the disease, as its retainers, always first assemble; till the malignant poison taint the blood and other juices; which being conveyed over the whole frame of the human fabrick, if not check'd, soon brings about its total putrefaction.

What Mr. *Becket* further adds in relation to this is, since we do not find, that the disease mentioned by *Gascoigne* was distinguished by any particular name; and that great numbers must have unavoidably died of it, at that time, from the imperfect knowledge of those who had the treatment of the first degrees of it; it must necessarily follow, that when the whole frame of the body had received a taint from the venereal poison; so as to occasion its breaking out into scabs and ulcers all over its surface, it must commonly be called by the name of some particular disease, whose appearances had somewhat of an affinity to it. Now if

we examine the nature of all the diseases that attack the human body, we shall not find the venereal malady, when it arrives at this state, to bear a greater resemblance to any other than the leprosy, as describ'd by the ancients: Nay, so great was the analogy between these diseases, that *Sebastianus Aquilanus* has endeavour'd to prove from *Galen*, *Avicen*, *Pliny*, &c. that the pox is only one species of the leprosy; and *Jacobus Cataneus*, a writer almost as early as the rise of the name of the pox, tells us, that it is not only possible there may be a transition from one of these diseases to the other; but that he saw two persons in whom the pox was changed into the leprosy; that is, from having large pustules on the surface of their bodies (from which the pox is denominated) to have become ulcerous or scabby. This particular state of the disease anciently put surgeons to a great deal of trouble: For, finding, that these ulcers were of a very obstinate nature, they were oblig'd to make use of great numbers of remedies, in order to conquer their bad disposition: But they observ'd, that all of them were useless, unless mercury was join'd with them: Now the dressing each particular ulcer being so very tedious, they order'd the patients to daub the ointments over the parts which were ulcerated; which done they were wrapp'd in linen cloaths till the next dressing: But after a few days they were extremely surpris'd to find their mouths begin to be sore, and that they spit very plentifully; but they tell us to their surprise, that in a little time the sores heal'd and the patients were cur'd: And by this accident it was, that the method of salivating by unction was first discovered, which is so much used among us at this day.

Now, tho' those foreign authorities above-mentioned might be look'd on as sufficient to prove, how our ancestors confounded these two diseases together; yet he proves from our own writers long before those, that tho' the pox was not only among us, but in distant nations anciently confounded with the leprosy; yet so exact were our writers in their observations of the infectious nature of one species of that disease, and in describing the symptoms, as was sufficient to enable any one to distinguish between them: He therefore, first inquires into the manner, how the leprosy was sometimes said to be gotten in those early times, and in the next place he examines the symptoms of the disease that attack'd the patient.

John Gadisden a very learned and famous physician, who flourish'd about the year 1340, in an excellent work of his entitled

entitled *Rosa Anglica*, speaking *de infectione ex coitu leprosi vel leprosæ*, has the following words; *primò notandum, quod ille qui timet de excoriatione & arsurâ virgæ post coitum statim lavet virgam cum aquâ mixtâ aceto vel urinâ propri intra vel extra præputium*: Speaking of the leprosy, he likewise recommends a decoction of plantain and roses in wine to be made use of by the woman, immediately after the venereal encounter; upon which he tells us she will be secure. Hence it is evident, that some of their leprous women (as they call'd them) could communicate an infectious malady to such as had carnal conversation with them; which proves, that the *pudenda* of the women must be diseas'd; forasmuch as we are absolutely assur'd, that infections of that nature only happen, when a sound part comes to an immediate contact with a diseas'd one; for, the symptoms always display themselves first in those parts thro' which the virulency is first convey'd. Now in a true leprosy we never meet with the mention of any disorder in those parts; which, if there be not, must absolutely secure the person from having that disease communicated to him by coition with leprous women; but it proves, there was a disease among them, which was not the leprosy, tho' it went by that name, and that this could be no other than venereal, because it was infectious: For, there is no other disease that can be communicated this way but the veneral; seeing the *pudenda* are only in that distemper so diseased, as to become capable of communicating their contagion.

The learned *Gilbertus Anglicus*, who flourish'd about the year 1360, reasons concerning the manner, how it is possible a man should be infected by a leprous woman; where if we allow him to call the malignant matter, which is lodged in the *vagina* (the *semen fœmininum*) we shall find, that he accurately describes the very first venereal infection, by part of the virulent matter being receiv'd into the *urethra*; from whence by means of the veins and arteries it is convey'd into the whole body, after which, says he, ensues its total corruption.

Let us now examine the symptoms of one sort of their leprosy; for, it must necessarily be divided into different species, when another distemper was blended with it, in which we observe such a diversity of appearances; and this Mr. *Becket* the rather does in this place, because it will furnish us with the next succession of symptoms after those already mentioned, as the venereal *ozæna's*, ulcers of the throat, hoarseness, the

proof

proof of its being communicable from the nurſe to the child, by hereditary ſucceſſion, &c. All which we find to be true in the venereal diſeaſe at this day.

Our countryman *Bartholomew Glanvile*, who flouriſh'd about the year 1360, in his book *De proprietatibus rerum*, tranſlated by *John Treviſa* Vicar of *Barkley*. in 1398 tells us, 'ſome leprous perſons have redde pymples and whelkes in the 'face, out of whome oftenne runne blood and matter: In 'ſuch the noſes ſwellen and ben grete, the vertue of ſmellynge 'faylyth, and the brethe ſtynkyth right fowle.' In another place the ſame author ſpeaks of 'unclene ſpoty'd glemy and 'quyttery, the noſethrilles ben ſtopy'd, the waſen of the 'voys is rough, and the voyce is horſe and the heere falls.' Among the cauſes of this ſort of leproſy, he reckons lying in the ſheets after them, eaſing nature after them and others, which the firſt writers on the pox look'd upon to be capable of communicating that contagion; 'alſo, ſays he, it comyth of 'fleſhly lyking by a woman, after that a leprous man hathe 'laye by her; alſo it comyth of fader and moder; and ſo this 'contagyon paſſyth into the chylde, as it ware by lawe of 'herytage; and alſo when a chylde is fedde wyth corrupte 'mylke of a leprouſe nouryce.' He adds, 'by whatever cauſe 'it comes, you are not to hope for cure, if it be confyrm'd; 'but it may be ſomewhat hidde and lett that it deſtroye ſo 'ſoone.'

Thus we ſee how our author, under the name of one ſpecies of the leproſy, gives a ſummary of the ſymptoms of the pox, and the ſeveral ways by which it is at this time communicated. Now when theſe two diſeaſes were anciently blended together, and paſs'd under the name of the leproſy only, this muſt be the real cauſe, why that diſeaſe ſeem'd to be ſo rife formerly; for, two diſtempers paſſing under one name muſt neceſſarily make it more taken notice of and much more frequent; not but that much the greater number of thoſe who were formerly ſaid to be leprous were really venereal, ſeems to be very evident; for, ſince that diſeaſe has been diſtinguiſh'd from the leproſy, it has drawn off ſuch vaſt numbers, that the leproſy is become a perfect ſtranger among us.

Thoſe acquainted with our *Engliſh* hiſtory, well know the great proviſion that was anciently made throughout all *England* for leprous perſons; inſomuch that there was ſcarce a conſiderable town among us but had a lazar-houſe for ſuch patients. In a regiſter belonging to one of theſe houſes, Mr.

Mr. *Becket* finds, that in *Henry* VIIIths time there were six of them near *London*, *viz.* at *Knightsbridge*, *Hammersmith*, *Highgate*, *Kingsland*, the *Lock*, and at *Mile-end*; but about 40 years before, he finds but four mentioned; and in 1452 in the will of *Ralph Holland* merchant taylor, register'd in the prerogative office, mention is made but of three, which, with his legacies to them, are as follows. *Item lego leprosis de* Loke *extra barram Sti* Georgii 20 s. *Item lego leprosis de* Hackenay (which is that at *Kingsland*) 20 s. *Item lego leprosis Sti* Egidii *extra de* Holborn, 40 s. From which it is worth while to observe, that the *Lock* beyond St. *George*'s church and that at *Kingsland*, are at this time applied to no other use than for the entertainment and cure of such as have the venereal malady.

Some of our learned antiquaries have been much at a loss to know the cause, why the leprosy should be so common in those early times, and so little known among us now: But Mr. *Becket* thinks it will be impossible to assign the reason, unless we allow, according to the proofs he has already adduced, that the venereal disease was so blended with it, as to make up the number of the diseased. It seems to have been the same thing with them in *France* as with us in *England*: For, the author of the history of that kingdom, published in *England* in 2 vol. *Octavo*, tells us, that the house of the fathers of the mission of St. *Lazarus* was formerly an hospital for leprous people; but that disease having ceased in this last age (since the pox has been distinguished from it) these lazar-houses have been converted to other uses; and Mr. *Becket* observes, that the writ *de leproso amovendo*, contained in the register of writs was (according to *Coke* upon *Littleton*) to prevent leprous persons associating themselves with their neighbours, who appeared to be so by their voice, their sores, the putrifaction of their flesh, and their stench. The method taken to prevent this noisome and filthy distemper, the leprosy, was castration which would infallibly prevent their getting the pox in the usual manner: It is certain that eunuchs are rarely or never troubled with the leprosy, according to M. *Le Prestre*, a Counsellor in the Parliament of *Paris*, who in *Cent.* 1. *cap.* 6. *de separatione ex causâ luis venereæ* writes to the following purpose, 'antipathy resists the poison of the leprosy; 'hence eunuchs and such as are of a soft, cold and effeminate 'nature are seldom or never infected with the leprosy; and such 'as are in danger of it, may, according to the opinion of phy'sicians, be castrated.' And *Mezeray* says, he has read in the life of *Philip* the *August*, that some men had such apprehensions

ſions of the leproſy (that ſhameful and naſty diſtemper) that to preſerve themſelves therefrom, they made themſelves eunuchs. Now it is highly probable, that thoſe who ſubmitted to ſo painful an operation, having before obſerved, that ſuch, as gave themſelves up to a free and unreſtrained uſe of women, fell at length under ſuch unhappy circumſtances, found that to be the only means to preſerve themſelves from it; which ſufficiently proves this ſpecies of the leproſy to be infectious; and for the reaſons before aſſign'd, it could be no other than venereal: For, how the true leproſy ſhould be prevented by ſuch means will be, Mr. *Becket* thinks, impoſſible for any perſon to determine.

There ſtill remains one very conſiderable ſymptom of the venereal malady to be taken notice of; becauſe it is looked upon to be the moſt remarkable in that diſeaſe, *viz.* the falling of the noſe: But ſince it has been already prov'd, that this diſeaſe (when it had arrived to ſuch a pitch, as to diſcover itſelf by thoſe direful ſymptoms, as are the immediate fore-runners of this) was by the ancients confounded with the leproſy, and called by that name, it muſt be among the ſymptoms of that diſeaſe we are the moſt likely to meet with it, if any ſuch thing as the falling of the noſe was known among them. Now the moſt likely method of coming to a certain knowledge of the infallible ſymptoms of the leproſy among the ancients, in its more confirmed ſtate, is to conſult the examinations thoſe unhappy perſons were obliged to undergo, before they were debarred the converſation of human ſociety and committed to cloſe confinement; and this Mr. *Becket* does from what remains he has met with in records and other ſcattered papers.

Firſt then, after the perſons, appointed to examine the diſeaſed, had comforted them, by telling them, that this diſtemper might prove a ſpiritual advantage; and if they were found to be leprous, it was to be looked upon as their purgatory in this world; and that tho' they were denied the world, they were choſen of God: The perſon ſuſpected was then to ſwear to anſwer truly to all ſuch queſtions, as ſhould be aſked. The examiners were very cautious in their enquiries, leaſt a perſon that was not really leprous ſhould be committed, which they look'd upon to be an almoſt unpardonable crime: They conſidered the ſigns as univocal, which properly belonged to that diſeaſe, or equivocal, which might belong to another; and did not upon the appearance of one or two ſigns determine the perſon to be a *Lazar*; and this Mr. *Becket* finds to have been the caſe of the wife of *John Nightingale* Eſq; of *Burntwood* in *Eſſex*, who in the

reign of *Edward* IV. *Anno* 1468 being reported to be a *Lazere*, and that ſhe converſed with perſons in public and private places, and did not (according to cuſtom) retire, but refuſed ſo to do, was accordingly examined by *William Hattecliff*, *Roger Marcal* and *Dominicus de Serego*, the king's phyſicians: But they upon ſtrict enquiry adjudged her not to be leprous, by reaſon the appearances of the diſeaſe were not ſufficient: Some of the queſtions put to the leprous perſons (as they called them) will more fully confirm what Mr. *Becket* has before advanced; he tranſcribed them from an old book of ſurgery, as follows; 'yf there were 'any of his lygnage that he knew to be *Lazares* and eſpecially 'their faders and moders; for, by any other of their kynred 'they ought not to be *Lazares*; then ought ye to enquire yf he 'hath had the company of any lepreſs woman, and yf any *La-* '*zere* had medled with her afore him; and lately becauſe of 'the infect matter and contagyous filth that ſhe had receiv'd of 'hym. Alſo yf his noſtrills be wyde outward, narrow within 'and gnawn: Alſo yf his lips and gummes are fowle, ſtynking 'and coroded: Alſo yf his voice be horſe, and as he ſpeaketh in 'the noſe.' Now the ſigns, which are here mentioned, were look'd upon to be univocal, and which made the examiners principally determine the perſons to be leprous; but what determinations any one would immediately give from ſuch ſymptoms at this time, no perſon ſure is ignorant of. But even theſe certain appearances would not always ſatisfy ſome perſons, if we may credit *Fœlix Platerus* in his medical and chirurgical *obſ. lib.* 3. who tells us, that ſome did not look upon them to be ſo, till the patients had an horrible aſpect, were hoarſe and their noſes fallen: Likewiſe in the *Examen leproſorum*, printed in the *De Chirurgia ſcriptores Optimi*, the author, ſpeaking of the ſigns of the leproſy, relating to the noſe, begins thus, *ſi nares exterius ſecundum exteriorem partem ingroſſentur, et interius conſtringantur & coarctentur; ſecundo ſi appareat cartilaginis in medio corroſio, et caſus ejus ſignificat lepram incurabilem.* And the above-mentioned *John Gadiſden* in his *chap. de lepra* ſays as follows; *ſigna confirmationis etiam incurabiliter, ſunt corroſio cartilaginis quæ eſt inter foramina, et caſus ejuſdem.*

Thus it has been proved, we had a diſtemper among us ſome hundreds of years before the venereal diſeaſe is ſaid to have been known in *Europe*, that was called the *Burning*; that this *burning* was infectious, and the firſt degree of the venereal diſeaſe; that this being common at that time, from their method of treatment, the pox muſt be unavoidable; that it had exactly the ſame appear-

nces it has now, tho' generally called by different names; that the ancients confounded it with the leprosy; that the vast number of leprous patients among us, before the venereal disease was distinguished from it; and the small number observ'd at this day, is a flagrant proof of the former; that in describing the symptoms of the leprosy, they give us those of the venereal malady; and by mentioning the manner of its being communicated, they describe that in which the pox is communicated at this day; that they recommended the same remedies to prevent the first attack of the leprosy, as are at this day in use to prevent the first symptoms of the pox; and that the falling of the nose, which is looked upon as the most remarkable symptom of the venereal disease, was anciently observ'd in what they call'd the leprosy in former ages.

An Account of the great Meteor that appeared on the 6th of March 1715—16; *by Mr.* Cotes. Phil. Trans. N° 365. p. 66.

THE appearance of the meteor was very nearly the same at *Cambridge* with what Dr. *Dannye* observ'd it at *Spofferth* in *Yorkshire*; only that the triangular streams of light were not so permanent; and the point to which they all converged was distant from the zenith about 20 degrees; its azimuth lying between the south and the east at about 10 degrees from the south, towards which point of the compass the wind tended: The position of this point of convergence may be accurately determined; for, at a quarter after seven, when the appearance at *Cambridge* was in its greatest perfection, it lay nearly in the middle between the two bright stars in the heads of *Castor* and *Pollux*. Mr. *Cotes* was inform'd, that some streams were observ'd to shoot forth immediately after sun-set, and that they did not entirely cease till about three or four in the morning.

It was after seven before Mr. *Cotes* had notice of this uncommon sight: At first he only saw two or three of the triangular streams towards the north and north-west; these were not of a long continuance, but were succeeded by others which appeared and vanish'd again by turns, arising from and ascending up to places in the heavens of very different altitudes above the horizon. From the time he began to view them, they began to ascend more and more copiously, being propagated still farther and farther from the north towards the west and east, and always directed towards the heads of *Gemini*;

till at length when they ſeem'd almoſt to meet at the point of convergence, they alſo began to aſcend up towards it from the ſouthern parts and all around it; inſomuch, that at a quarter after ſeven, there was a perfect canopy of rays: The bottom of this canopy did no where reach down to the horizon; for, near the north, where it deſcended moſt, its altitude was about 10 or 15 degrees; and near the ſouth where it deſcended leaſt, its altitude was about 40 degrees: It remain'd in this ſtate about two minutes, during which time ſeveral colours were obſerv'd; ſome fainter and ſome more permanent; others brighter, but quickly vaniſhing: Thus in the weſt Mr. *Cotes* obſerv'd the rays to be tinged for ſome conſiderable time with an obſcure and heavy red; and in one of the brighteſt ſtreams at another time, there ſuddenly broke out a very vivid red, which was inſtantly and gradually ſucceeded by the other priſmatic colours, all vaniſhing in about a ſecond of time: Theſe colours affected the ſenſe ſo ſtrongly, that he thought them to be more intenſe than thoſe of the brighteſt rainbow he had ever ſeen. A little time before the appearance loſt its perfection, a ſhaking and trembling of the ſtreams was obſerv'd, chiefly in their upper parts; during which, their convergence was confounded, and the whole heaven ſeem'd to be in a convulſion: At the ſame time Mr. *Cotes* could perceive waves of light towards the north, which mov'd upwards, and in their motion croſs'd the ſtreams, lying parallel to the horizon; theſe waves were different from thoſe broad ones, obſerv'd in *Yorkſhire*, and which Mr. *Cotes* likewiſe took notice of; their breadth ſeemed to be about a degree, their length about 90 degrees; and they reſembled thoſe ſlender waves on the ſurface of ſtagnant water, caus'd by caſting in a ſmall ſtone.

About ſeven or eight years before, Mr. *Cotes* obſerv'd this other meteor; along the horizon in the north, there lay a white and luminous, and ſeemingly denſe matter in the form of a cloud, repreſented by *a b c d* Fig. 7. Plate II. its length *a b* was about 10 or 15 degrees; from this there aroſe directly upwards, pointed ſtreams of the like luminous and white matter, which yet did not appear in any part thereof to be ſo denſe as the former; and it became gradually more and more rare in its upper parts, ſo as to vaniſh almoſt inſenſibly at the points: There was ſome little difference in the height of theſe ſtreams; but they generally aſcended up to about four degrees above the horizon; they were very numerous and contiguous

uous to each other, and ſeemed to be compos'd of very ſlender parallel filaments or rays. This was the common appearance; nd the only remarkable thing he farther obſerv'd was, that ometimes a fire or flame would break out in the cloud *a b c d*, nd move along it in a direction, parallel to the horizon; and uring this meteor, a pointed ſtream directly over the fire eemed to run along with it, and to paſs by the other more ix'd ſtreams, to which it always kept itſelf parallel.

Mr. *Cotes* is perſuaded, that the appearance of *March* 6. was of the ſame kind with this; for, let A B Fig. 8. repreſent he plane of the horizon; C the place of the ſpectator; E F a und of vapours or exhalations at a conſiderable height above s, diffus'd every way into a large and ſpacious plane, parallel to the horizon: This fund of mix'd matter will by fermenation emit ſtreams from itſelf, ſuch as E G, F H, &c. which, f the wind be perfectly ſtill, will aſcend perpendicularly upwards; if it be boiſterous and irregular, they will be blended nd confounded together; but if it be very gentle and uniorm, as it was at the time of the appearance, they will be nclin'd towards the point of the horizon, which is oppoſite to hat from which the wind blows: Now if A D B repreſent the oncave of the heavens; and a line C D be drawn parallel to he columns E G, F G, &c. it is certain by the rules of perſpective, that theſe columns will appear upon that concave to onverge all around towards the point D; thus the column E G will ſeem to ariſe from the point *e*, to aſcend up to *g*, and o take up the ſpace *e g*; and in like manner the arch *f h* will be the projection of the column F H: Hence it is evident, hat the reaſon why the triangular ſtreams aſcended at firſt only rom the northern parts of the heavens was, that the fund of matter E F was not yet arriv'd in its motion to the line C D; fter it had paſs'd that line, it is plain, they muſt appear to ſcend from all quarters: A great number of columns being, herefore, diſpos'd to emit light at the ſame time, caus'd that erfect canopy, deſcrib'd above. The reaſon why that canopy deſcended lower in the north than in the ſouth was, that the ſhining columns, which had not yet paſs'd the line C D, were more numerous and more remote from it than thoſe which had paſs'd it; for, if the point E be farther diſtant from C D than the point F, the arch A *e* muſt needs be leſs than the arch B *f*: An irregular guſt of wind blowing upon and ſhaking the columns was, Mr. *Cotes* ſuppoſes, the cauſe of that trembling, which

which appeared in the triangular ſtreams, and likewiſe th cauſe which deſtroy'd that fine appearance of the canopy: Th ſlender circular waves, obſerv'd at the ſame time, might like wiſe be explained from the ſame cauſe.

In order to repreſent this appearance in ſome tolerabl meaſure, take a hoop, and round about it faſten ſevera ſtreight ſticks parallel to each other, but all inclin'd to th plane of the hoop; hold this plane parallel to the horizon, an in that poſition move it with ſticks over a candle, the ſhadow of the ſticks upon the ceiling of the room will converge to point, not directly over the candle (as they would have done had the ſticks been perpendicular to the plane of the hoop) bu to the point, in which a line drawn from the candle, paralle to the ſticks, ſhall interſect the plane of the ceiling.

An Account of the Operation of Medicines by Dr. John Quincy. Phil. Tranſ. N° 365. p. 71.

AS to the operation of medicines and particularly o purges, Dr. *Quincy* ſuppoſes; 1. That all thoſe parts o an animal body, that are vaſcular, or thro' which any flui paſſes from the inteſtines to the minuteſt fibre, are the ſea of the operation of a medicine. 2. That this whole courſe o circulation or animal motion is naturally diſtinguiſh'd into three different ſtages, according to the different capacities o the veſſels, and the motions of their contents; each having it proper out-let, and that theſe are the ſeat of the three con coctions, ſo often mentioned by phyſical authors; the firſt i the ſtomach and bowels, having the *anus* for its emunctory; the ſecond all within the circulation of the blood, ſo far as it retains its colour, having the kidneys; and the third all beyond that circuit, having the ſkin for its emunctory. 3. That every medicine, which cauſes evacuation, is a purge. 4. That every purge operates as a diſſolvent, by fuſing the juices, and increaſing the quantity fit for expulſion; or as a *ſtimulus* by accelerating their motions, ſo as to bring the matter fit for expulſion oftner to the ſecretory out-let; or both.

Theſe aſſumptions are premiſ'd only for the better proof of this grand propoſition; *viz.* that a change in the bulks, figures and motions of the component particles of a purging medicine will change the ſeat of its operation, and fit it for exertion in larger or leſſer veſſels, as thoſe mechanical affections are intended or remitted.

For

For illustration hereof, it may be convenient to attend the ma-
gement of common practice, in making a purge operate more
less than it otherways would do.

Substances that are gross and heavy, as those chiefly consisting
saline and earthy particles, such as tartar, manna, &c. if they
e reduced smaller by triture or repeated solutions, operate
ore gently; but if sharpened by acids or any management that
poses their angles more sensibly to the membranes, they are
ugher and take place sooner.

Resinous medicines as scammony, gamboge, jalap and most
vegetable production are more violent and operate sooner,
hen they are more tenacious and adhesive, as in their extracts;
t gentler, when divided by hard brittle substances, such as
lt of tartar, sugar, &c.

Medicines, that have sulphur and salt in their composition, are
ore or less rough and speedy in their operation, in proportion
their greater or less participation of the saline ingredient and
e asperity of its angles; of this kind are most minerals and
eir preparations; and it may be sufficient to instance in the
anagement of antimony and mercury; the first of these is by
emical analysis known to be a composition of a subtile sulphur
d salt, and the more the saline part is let loose by preparation,
d opening the sulphur, as it is commonly called, the more ve-
emently and the sooner will it operate; whereas, in its lesser
eparations, when the salts are closely wrapped up in their na-
ve sulphur, they will hardly work at all, till they are got into
e farthest stages of circulation; mercury *per se* is little known
a medicine; and its first preparation, which makes it into sub-
mate, so loads it with saline *spicula* that it amounts even to a
ison; but the more those *spicula* are broke by triture, sublima-
on, &c. the milder it operates; and if to the comminution of
s points there be added a sulphur subtile enough to join it, it
ay be reduced to so mild a medicine, as not to be felt, but in
e last stage of operation.

This short view may be sufficient to shew, that it is excess of
perity and motion in a medicine, that will not suffer it to pass
e stomach, without irritating it into such convulsions, as will
row it up again by vomit; that a farther comminution and
oothing its figure will admit it into the bowels, and make it
perate by stool; that a still farther remission of its properties
ill convey it into the blood, and make it there promote evacua-
on by urine; and that a still farther comminution will convey
into the minutest canals, where by the same properties, only in
a lower

a lower degree, it will increaſe perſpiration or cauſe ſweat: S
that the ſubtiler medicines operate in the capillaries and ſmalle
fibres by the ſame mechaniſm, that more groſs medicines do i
the common ſtream of the blood, when they go off by urine; o
the groſſeſt of all do in the larger paſſages, when they promo
ſtool.

Hence the ſkill of preparing and adminiſtering medicine con
ſiſts in proportioning its manifeſt and known properties to the ca
pacity and circumſtances of the part it is to operate in; and t
intend or remit its mechanical affections, as it is ſooner or late
to take place in the larger or ſmaller veſſels.

Of the firſt claſs there are few to be reduced ſmall enough t
go beyond the larger paſſages, and none of them are worth th
pains they require to fit them farther than for diuretics; beſide
their natural fitneſs to attract and join with the ſerous part of th
blood, whenever they get into that ſtage of motion, runs them
off by the kidneys, before they can undergo comminutio
enough to get farther; but if by frequent repetitions of ſuch me
dicines and uncommon laxity of the paſſages, any thing be paſſe
into the habit, their groſsneſs fouls the delicate ſtrainers, which
are left for their expulſion; and they lodge upon the glands and
capillaries in ſuch manner, as induces intermittents, which is ob
ſervable in ſeveral patients after the uſe of cream of tartar, th
common cathartic ſalts and the purging waters; eſpecially at th
latter end of the ſummer, when the heat of the preceeding ſea
ſon has debilitated the ſolids and left them under too great a re
laxation.

Among the reſinous purges, there are ſeveral very powerfu
ones; but where their operation is propoſed in the *viſcera*, bloo
and remoter parts, they muſt be exceedingly divided; and thi
we find ſpirituous *menſtruums* will do by taking up the moſt ſub
tile parts only, and conveying them into very ſmall paſſage
where their operation is chiefly by fuſion, becauſe the ſoftneſs o
ſuch ſubſtances cannot make them, hardly in any degree, to ac
as *ſtimuli*, farther at leaſt than ordinary detergents: And thus we
find aloes, the chief of this tribe, to go farther into the habit
and be longer before it operates, when managed in a ſpirituou
menſtruum, as in the *Tinctura ſacra*; the *Rad. Turpeth*: And
likewiſe *colocynth*, with all of the vegetable kind, that will yiel
to a ſpirituous liquor, are to be conveyed by that means into th
fartheſt ſcenes of animal action; and there prove efficacious me
dicines in caſes, that with other management, they would neve
be able to reach: And on this foot it undoubtedly muſt hav

been

en, that in practical writers we meet with many of this ſort
entioned, as alterants; the *colocynth* particularly by *Helmont*;
r, they commonly compriſed under that general appellation all
edicines that operated in the remoteſt paſſages.

But the moſt efficacious purges and thoſe which require the
oſt ſkill are from the mineral kingdom; theſe abound in ſoli-
ty beyond any other material; and therefore, whereſoever
ey are brought into action, excel in quantity of impulſe: Se-
ral of theſe, therefore, require not only the utmoſt comminu-
on to convey them into the farther ſcenes of operation; but alſo
me reſtraint to their aſperities and motions, to fit them for ſe-
ral intentions: Thus ſublimate is not only to be much ſweet-
ed, that is, ſmoothed in its points, to make it a ſafe purge in
e larger veſſels; but if it is intended to go farther than the
ood, and thoſe glands, which in that circuit, they are moſt apt
be lodged upon, when it ſalivates, it muſt be rendered not
ly very fine, but covered with ſuch ſubſtances, as weaken its
oints and make it ſtrain into the laſt ſubdiviſions of the conſti-
tion. To this purpoſe, the common practice wiſely contrives
diſtempers that lie fartheſt off, according to the courſe of cir-
lation, to wrap up the baſis of this medicine in ſulphurs and
ch like ſubſtances, as follow it into its laſt diviſion, without
nparting to it any aſperities to make it act as a *ſtimulus*. Thus
r all cutaneous foulneſſes and habitual taints, the cinnabar, the
hiops and all of that ſortment are in readineſs; and that ordi-
ry ſulphurs will cover and deaden the efficacies of mercurial
eparations; ſo that they ſhall not operate, but in ſuch parts
ly and in certain circumſtances, is demonſtrable in ordinary ſa-
vations, which are to be lowered at pleaſure by ſulphureous
edicines.

Medicines from ſuch minerals, where a ſalt and ſulphur are
nited by nature; as they are in ſome mercurials by art, as anti-
ony, native cinnabar, ſteel, *&c.* are manageable only upon the
me principles; and the more they are deſigned to be carried
to the habit, the more are they to be reſtrained by their natu-
l or adventitious ſulphurs: Steel, when opened by and united
ith the points of acid liquors, operates ſooner, and will ſome-
imes prove emetic; but when it is covered with an additional
lphur, it will go farther, and anſwer intentions much more re-
ote, as is manifeſt in the common preparations of ſteel with
artar or vinegar, and with ſulphur.

This way of thinking on theſe occaſions ſeems the more juſt,
rom conſidering the texture of thoſe ſubſtances, which by a na-

 tural

tural preparation are fitted for operation in the minuteſt part of an animal body; ſuch as thoſe of the aromatic kind; all which more or leſs, according to their greater or leſſer degree of ſubtilty and ſmoothneſs promote a *diaphoreſis:* They conſiſt of exquiſitely fine ſalts, covered with a very ſubtile ſulphur, as is demonſtrable by chemical analyſis; and the common *ſal volatile oleoſum* is an admirable contrivance upon the ſame foundation, where a very volatile animal ſalt is covered with a moſt exalted vegetable oil, whereby it is adapted to paſs into the minuteſt fibres, and make, as it were, a part of the animal ſpirits themſelves.

And here it may not be amiſs to obſerve, that all animal ſalts are very volatile, or are eaſily rendered ſo; but when bare and naked, juſt as extracted by the fire, with a mixture alſo of its own particles in their compoſition, they are too pungent to be felt without painful ſenſations; but when ſoftened with a fine portion of an oppoſite texture, which is ſmooth and yielding, they become moſt efficacious and ſafe ſudorifics.

On theſe conſiderations we are no longer ſurpriſed at the ſubtile ſalts of *cantharides* more ſenſibly affecting the bladder than any other parts, and why camphire prevents thoſe injuries; for the exquiſite ſmallneſs of thoſe *ſpicula* makes them imperceptible, but in the moſt minute canals, into which the fibres that compoſe the membranes of the bladder are known to be divided; and camphire blunts their irritations, becauſe its exquiſite ſubtilty enables it to follow them into thoſe meanders, and ſheath their aſperities.

To this purpoſe it is very remarkable what ſeveral commonly practiſe in guarding even mercurials againſt their ſtimulating properties, and ſending them into the fineſt paſſages to operate by fuſion, and the bare force of impulſe; for, not only calomel and the *mercurius dulcis* may be reſtrained from manifeſt operation in the wider paſſages and the glands about the mouth; but even mineral turbith, which of itſelf in a ſmall doſe will operate powerfully by vomit and ſtool, when mixed with camphire, will not be ſo much felt in thoſe reſpects, but go into the fartheſt circuit of motion, and promote the cutaneous diſcharge in a more efficacious manner than any medicine of leſs ſpecific gravity; but in this management the camphire is to be mixed but very little before taking, otherways it hath not this effect, which appears to be from its great volatility, that makes it in a great meaſure exhale, while it ſtands mixed in a medicine.

This theory is applicable to ſeveral good purpoſes in practice; and this only inſtance of camphire is ſufficient to ſuggeſt to ſuch as

turn their thoughts this way, in what cases that and such substances of like subtility and texture may be used with success; for, e seat and causes of many chronic distempers lie very remote the course of circulation, and the reason why they elude the rdinary means of cure, it is probable, is owing to the want of fficient attention to that particular management of efficacious medies, which is necessary to carry their operations so far: An rdinary judgment knows how to intend or remit the efficacy of edicines, by sharpening their points and increasing their quanty of impulse; or softening and weakening them with broths d the gross-expressed oils of almonds or linseeds in the first d larger passages; but an active medicine, or a distempered ritating salt in the minutest capillaries and fibres, is not to be anaged by such coarse methods.

An Account of an extraordinary Cramp *and* Fistula; *by Dr.* Steigerthal. Phil. Trans. N° 365. p. 79.

JOHN *Henry Oizmann*, aged 31 years and born at *Barum*, was 15 years old, when the following misfortune befel him. He felt a *spasmus* or cramp in his left hip and the lower part his leg; as this pain seised him pretty often, he consulted Mr. *Raek* a surgeon at *Ulzen*, who applied several plaisters to e part affected; but without any relief to the patient: After l those fruitless efforts, the surgeon made about 37 incisions over e patient's whole leg (which to outward appearance was become ry brown) of which he was not at all sensible, unless when the strument happened to grate upon the bone; the *periosteum* beg still sound: His leg however grew daily blacker, and the ain continued both in the *periosteum* and in all the bones of the pper and lower part of the leg: At last a black circle was obrved round about the muscles of the hip, as an indication of approaching mortification. This circle appeared so visibly, if it had been separated with a knife from the other part. It ver after spread, and came to such a head, that without any ther help and cure, the flesh began gradually to rot away from e bones; and at last quite fall off from the upper part of the g, which preserved its soundness; after this nothing was seen, but e bare tendons or sinews hanging down like so many strings r cords; there also remained a piece of the inferior muscles of e hip fastened to the superior part. At last the tendons, beoming dry, consumed away; and after all, the leg itself, to wit, e *os femoris* dropped off in such a manner, that there remained bout four inches between the bones and the flesh, loosely hanging

down from them. The fleſh at laſt grew up to the bone without any manner of help, and faſtened itſelf to the bone; and in this ſound part the patient felt a great pain, wheneve the weather proved tempeſtuous. It was remarkable, that a the ſame time the patient alſo perceived a ſwelling in the *tar ſus* of the right foot, the matter of which diſcharged itſel thro' the toes and was of ſo corroſive a nature, that it ha conſumed all the toes but the little one. The ſurgeon a length healed up the wound; but after all, there was ſtill bu little feeling or warmth in the foot.

This man was afterwards married to a woman 41 years ol whoſe bodily conſtitution was almoſt as remarkable: In he younger years ſhe was gored by a wild boar under the ſho ribs of the left ſide; this wound became fiſtulous, and wh food ſhe eat was diſcharged, half concocted thro' this apertur ſo that you might diſtinguiſh what ſort of food it was; how ever, notwithſtanding this, ſhe had her daily evacuation *pe anum.*

An Experiment to prove that there is an interſperſed Va cuum; *by Dr.* Deſaguliers. Phil. Tranſ. N° 365. p. 81.

THAT bodies of the ſame bulk do not contain equ quantities of matter; and therefore that there is an in terſperſed *vacuum* Dr. *Deſaguliers* proves by the following ex periment.

He took three pounds of mercury, which filled thrice ſmall glaſs jar exactly full, and he poured it into a thin *Floren* flaſk: Afterwards having poured the ſame quantity of wat (that is three of the ſame jars full) into another ſuch flaſk, h ſet both the flaſks in a pail and pouring boiling water abou them, keeping the flaſk that had the water forcibly dow that it might ſtand as low in the hot water as the mercury After the fluids in the flaſks had received a ſufficient degree o heat from the water, for the ſpace of five minutes; he too the flaſks out of the hot water, and putting that which hel the water into a cylindric veſſel that had three pints of col water in it, he at the ſame time plunged the flaſk with mer cury into another cylindric veſſel, that alſo contained three pin of cold water; and he obſerved which of the cold waters wa moſt heated in the following manner.

Upon holding a little thermometer in the firſt veſſel of col water, ſo as to have its ball covered with the water; whe the flaſk of warm water was put in, the ſpirits roſe two de grees

ees; then putting the thermometer into the water where the ask of mercury stood, the spirits rose three degrees higher: he thermometer being again put into the first vessel, they ll four degrees; and afterwards again into the last, they rose lmost three degrees.

This shews, that more heat is communicated by warm mercury than by an equal bulk of water equally heated; and herefore, that there is more matter in the mercury; but how uch more, is not determined by this experiment alone.

N. B. The warm mercury and warm water were not oured into the cold; but only communicated their heat thro' he flasks.

n Account of a Contagion *among the* Cattle *in the* Venetian Territories *in Autumn* 1711; *by Dr.* Michelotti. Phil. Trans. N° 365. p. 83.

DR. *Michelotti*, being in the *Venetian* territories about *October* 1711, took that opportunity of making a particular enquiry into the circumstances of the mortality that eigned at that time among the black cattle; and was himself n eye-witness of the greatest part of the facts contained in his account, and received the rest upon the spot from persons f integrity and credit.

Almost all the sick cattle refused every sort of food and rink; they hung their heads, had shiverings in their skin and n their limbs; they breathed with difficulty and their expiraion in particular was attended with a sort of rattling noise; hey were so feeble, that they could hardly go or stand upon heir legs: Some few of them eat a little and drank very much; others had fluxes of excrement variously coloured, of a very offensive smell and frequently tinged with blood: Several of them had their heads and bellies swelled in such a maner, that upon clapping them with the hand on their paunches, or along the *vertebræ* of the loins, they sounded like a dry bladder, when full blown: In some the urine was very turbid, in others of a bright flame-colour. In comparing the pulses of the sound cattle with those of the diseased, the Dr. found the latter to be quicker and weaker: There was but little heat perceivable by the touch in any of them; their tongues were soft and moist, but their breath exceedingly offensive: Besides these particulars, he was informed by those who attended the sick cattle and other persons of credit, that in some of them they observed crude tumours in several parts of the

body,

body, as alſo watery puſtules and diſorderly motions of th
head, with dry, black and fiſſured tongues, that in others o
them they met with tumours that came to maturation; wit
putrid matter iſſuing from the mouth and noſtrils; worms i
the *fæces* and in the eyes; bloody ſweats and ſhedding of th
hair.

In comparing the fleſh of the cattle, that died of this diſtem
per, with that of others killed for the market, the Dr. foun
the muſcles in the former, which lay immediately under th
ſkin, to be ſomething livid: Having opened the three caviti
of the body, he applied himſelf with the utmoſt diligence t
examine the brain with its membranes, the *trachæa*, *oeſoph*
gus lungs, and heart with its auricles, the *vena cava*, *aort*
the diaphragm, the liver, ſpleen and other parts of the low
belly; and in all theſe there was no diſcernible differenc
either as to figure, ſize, contents, ſituation or connection wit
the neighbouring parts, from what was obſerved in ſound ca
tle killed by the butchers, excepting the particulars hereaft
mentioned. The blood found in the ventricles of the hea
the pulmonary veſſels, the *aorta* and *cava* tho' ſtill warm, wa
conſiderably blackiſh and almoſt coagulated. In opening th
upper and middle cavity, the ſcent was offenſive, but tole
able; whereas that, proceeding from the lower belly, wa
quite intolerable. In ſome few carcaſes the *viſcera* differe
from what they are in a natural ſtate, with regard to thei
ſize, conſiſtence, contents, colour and ſmell: In ſeveral th
paunch was found very much contracted and dried, with a ha
ſubſtance contained therein: In others the lungs were ſwelle
and livid, the liver tumefied, and the brain watery an
putrid.

The Dr. having ordered ſeveral of the ſick cattle to b
blooded, found, that the blood did not iſſue out of the veſſel
in a continued ſtream, as uſual, but with a broken and in
terrupted flux, one part thereof not immediately ſucceedin
another. Having cauſed the blood to be received in prope
veſſels, and let it ſtand for ſome time, he found it entirely coa
gulated, without any ſeparation of the *ſerum*, and attached t
the ſides of the veſſels, with a reticular pellicle upon the ſur
face, expoſed to the air. All the cattle that were blooded
being 18 in number, died in a few days after the bleeding, ex
cepting one only, in which the vein was opened upon its be
ing firſt taken ill.

Havin

Having enumerated all the symptoms of the disease; the Dr. ncludes from the whole, that this sickness among the cattle s a malignant pestilential fever, killing almost all those that re infected with it.

The immediate cause of this he takes to be a preternatural ickness of the blood, occasioned by a coagulation beginning those parts of it which constitute the *crassamentum*; where- the globules of the blood and the particles of the *serum* re imprisoned in a sort of *reticulum*, formed by the union the fibres of the blood.

The occasional cause of this sickness he deduces from the ld and wetness of the season, that prevailed all the preceed- g year from *October* 1710 to *November* 1711; which observa- n is worthy of remark, since the season, preceeding the mor- ity among the cattle in *England*, was remarkably dry, and t the symptoms of the distemper agreed with those observed *Italy*, as appears from Mr. *Bates*'s account in *Phil. Trans.* 358.

Portion of the Colon *hanging out thro' a Wound for* 14 *Years; by Dr.* Abraham Vater. Phil. Trans. N° 366. p. 89. *Translated from the* Latin.

ONE *George Deppe*, a native of *Halberstadt*, and 34 years of age, by a wound in the left *hypochondrium* in battle of *Ramillies* in 1706, had for 14 years a large por- n of the *colon*, by this means cut and inverted, hanging out his body, a span in length.

This intestine coming out thro' the wound, that at the ne time penetrated into it was turned inside out; and co- ring thus in the middle formed two portions; one of which tending upwards had an orifice, regarding the thin guts, and charging the *fæces*; the other, hanging downwards had an fice opening into the *rectum*, so as to discharge by the *anus* glyster injected into it: On its inner surface, now turned twards, a great number of glands, of a white and cineritious our appeared, prominent like warts, and affording a very reeable sight; these, when roughly handled, yielded blood: e intestine thus hanging out was never wholly retracted o the *abdomen*, tho' it was a little retracted, when the sto- ch happened to be empty; but when full, it was farther truded, especially, on retaining his breath. He used with- t any inconveniency the coldest water, nay that mixed with and snow, to wash off the filth; and the intestine could

bear

bear the coldeſt air, only that it made it ſhrink, grow hard and ſomewhat pale: Any ſort of food agreed with him; yet new fruit and greens did not incorporate with his other victuals, but were diſcharged undigeſted; as alſo broths taken without ſolid food.

ABCD Fig. 1. Plate III. repreſents the *colon* turned inſide out, hanging out at the wound, and reaching upwards and downwards; *a* the ſuperior orifice opening into the thin guts, thro' which the *fæces* were diſcharged; *b* the inferior orifice, leading to the *rectum*, thro' which a glyſter was diſcharged by the *anus*; *c d* the veſtiges of the wound enlarged by the ſurgeon, thro' whoſe middle the inteſtine hung out; *e* the navel; *fff* great numbers of glandules; *g* the inguinal region; *hh* the *dorſum*; *i* the *coxendix*; K the lumbar region.

Obſervations on the Bones *and* Perioſteum; *by M.* Leewenhoeck. Phil. Tranſ. N° 366. p. 91.

M. *Leewenhoeck* found by frequent obſervations on bones, that their ſuperficial part conſiſts of a vaſt many ſmall veſſels, and ſome few of a larger ſize; which laſt, when they came to the ſurface of the bone, appeared to him inveſted either with a membrane or bony ſubſtance, perfectly tranſparent.

M. *Leewenhoeck* once diſcovered in a ſmall portion of a ſhin-bone four or five veſſels of ſuch a ſize, that a ſingle filament of ſilk might have been drawn thro' their aperture: One of theſe appeared to him to conſiſt of two openings; each of which ſeemed to be furniſhed with a valve, diſpoſed in ſuch a manner, as to let out what was contained in the veſſel, but ſuffer nothing to enter it.

As to the matter that iſſues out of the bones and is convey'd into the *perioſteum*, M. *Leewenhoeck* diſcovered its ſource to be in the ſpongy or cellular ſubſtance on the inſide of the bone; which is the repoſitory of the marrow.

This ſpongy ſubſtance conſiſts of long particles cloſely united and linked together, which are compoſed of a vaſt many ſmall veſſels, ſome running lengthways, and others taking their courſe towards the ſides of the bony particles; which laſt, notwithſtanding their great number of apertures, are yet exceeding hard, ſome of them lying parallel and others perpendicular to the length of the bone.

Thoſe particles, that lie perpendicular to the length of the bone, have veſſels proceeding from their extremities; and other veſſels that compoſe the *cortex* or ſuperficial part of the bone

bone proceed from their ſides, where they do not lie cloſe together; and thoſe long particles, that lie parallel to the length of the bone, emit veſſels from their ſides, that iſſue out thro' the ſide of the bone: It is impoſſible to conceive the prodigious number of ſmall veſſels of which the cortical part of the bone conſiſts, which in ſome places lies no thicker on the ſpongy part of the bone than a thick hair of a man's head; tho' in other places it has three or four times that thickneſs.

The *perioſteum* is united to the *cortex* of the bone, not only on the outſide, but even by entering in ſeveral places into its very ſubſtance, and is joined thereto by the veſſels, which iſſue out from the bone, in ſuch a manner, that ſometime one cannot determine which is the bone, and which belongs to the membrane inveſting it, they both appearing in the microſcope to conſiſt alike of exceeding ſmall veſſels.

Fig. 2. Plate III. repreſents a ſmall part of the bone with the *perioſteum* adhering thereto; ABCDEF the bony part; BGHIE the *perioſteum*, whoſe thickneſs is repreſented by BG or IE; tho' in other places of the bone, and even at no greater diſtance than two or three hairs breadth, it is twice or thrice as thick; all the ſmall veſſels in the *perioſteum* are repreſented by ſo many dots or points; but in other places, where M. *Leewenhoeck* had obſerved the membrane of twice this thickneſs, its upper part has appeared of a different make from the under; inaſmuch as he could diſcover in the upper part not only thoſe veſſels that had been cut tranſverſely, and conſequently, repreſented by ſo many points; but likewiſe a great number of other veſſels running lengthways along the membrane, as repreſented by LOPQNM in Fig. 3.

M. *Leewenhoeck* is fully perſuaded, that the part repreſented by BGHIE in Fig. 2. is not entirely membranous, but that ſome part of it is really bony. Upon cutting thro' the *perioſteum* ſo deep, as to divide the part of the bone, marked ABCDEF in the ſame Fig. we find the ſame appearance of pores in the bony ſubſtance, which are no other than the tranſverſe ſections of ſmall veſſels; and beſides theſe, there are other veſſels running lengthways in the bone; and we obſerve juſt the ſame in thoſe tranſparent parts, that lie between the bony particles, which are repreſented thicker between BCDE than they appeared to M. *Leewenhoeck*.

M. *Leewenhoeck* ſuppoſes, that the uſe of theſe bony particles is to convey an oleaginous liquor into the *perioſteum*, and from thence by the intervention of the other membranes into all parts of the body, in a healthful ſtate.

In another place he obſerv'd a great number of veſſels ariſing from a greater depth within the bone, which drew cloſer together, ſo as to compoſe ſmall *faſciculi*, before they enter'd the *perioſteum*, in which they ſeparated from each other and diſpers'd themſelves again: It is hard to determine, whether theſe veſſels convey any liquor into, or from the bone; tho' M. *Leewenhoeck* rather thinks they ſerve for the latter purpoſe.

Having placed before the microſcope another piece of bone with the *perioſteum* adhering thereto, he could diſcover a great number of veſſels, cut thro' lengthways, as they ran along the *perioſteum*; and others cut thro' tranſverſely, appearing like ſo many points, as repreſented by KLOPQNA in Fig. 3. where the bony part is mark'd KLMNA, in which tho' no pores or veſſels are here repreſented; yet it is full of openings: That part repreſented by LOPQNM is not entirely membranous; for, M. *Leewenhoeck* ſuppoſes, that that part of it, which lies next the bone, and is repreſented by LMN, is of a bony ſubſtance.

He had another ſmall piece of bone, lying before a microſcope, part of which he caus'd to be repreſented by RSWXTV Fig. 4. RSTV is the bone; SWXT the *perioſteum*, which in this place was no thicker than a thick hair of a man's beard; but in another part of the ſame bone at a ſmall diſtance it was full four times that thickneſs.

M. *Leewenhoeck* placed before a microſcope another piece of bone in ſuch a manner, as that the bone did not appear; but only the *perioſteum* and muſcular fibres, which were cut thro' tranſverſely and appeared to be ſurrounded by the fibrils of the *perioſteum*, as repreſented by YZCDAB in Fig. 5. where YZAB is the *perioſteum* and ZCDA are the fleſhy fibres cut thro' tranſverſely: This piece of bone was taken from one of the ribs of a fat ox; and he was ſurpriz'd, that in this place, as he cut thro' the rib lengthways, he could not diſcover any particles of the marrow; whereas in other parts the rib abounded with them.

Notwithſtanding the great number of obſervations M. *Leewenhoeck* made on the bones, and the membrane inveſting them, *viz.* the *perioſteum*, he could not ſatisfy himſelf entirely about them: He ſtill imagin'd, that the part of the *perioſteum*, which immediately covers the bone and is cloſely united thereto, muſt have a degree of hardneſs approaching to that of the bone; and that at a ſmall diſtance from the bone, the *peri-*

periosteum must have a softness and flexibility like that of the carneous and adipose membranes.

M. *Leewenhoeck* had kept four pieces of the ribs of a fat ox full two months, which were then become very dry: From one of these he tore off the *periosteum*, which he found stick harder to the bone than he could have imagin'd; and he observ'd that a great many particles of this membrane still adher'd to the bone. He did this with a design to make some observations on the superficial part of the bone, which is not near so hard, as those bony particles that lie a little deeper: From this bone he cut off some very thin slices, both lengthways and transversely; one of which he placed before the microscope and caus'd it to be delineated.

This piece is represented by ABKC Fig. 6. having been cut off transversely, and as thin as possible, from the rib together with part of the *periosteum*, as from K to C, still adhering to the bone, and another part torn off from the bone, as represented by BKD, only that in some places the bone and membrane are still united by vessels torn out of their places, and that run from one to the other.

In this figure DEFC represents the *periosteum*; the part design'd by EGHIF in some measure lies upon it, which M. *Leewenhoeck* did not know what to make of, tho' it appear'd to him to be membranous.

He had likewise some very thin slices shaven off from the rib both of an ox and a calf, from which he tore off the *periosteum* entirely, or at least as much as possible; afterwards he caus'd the edge of the bone, it had stuck to, to be represented by the crooked line LMN Fig. 7.

OPQ Fig. 8. represents the edge of another small slice of bone, from which the *periosteum* had been torn off; by which appearance it should seem, that the union of the *periosteum* with the bone is so firm and strong, that in separating it, some of the superficial particles of the bone are torn off with it.

He likewise discover'd some vessels running along within the marrow-bone of the shank of an ox, that seemed to be blood-vessels.

Since it appears from M. *Leewenhoeck*'s observations, made with the utmost diligence and care on bones of all kinds, that they do for the most part consist of exceeding small vessels, which arise from the inner, hollow or spongy part of the bone, and passing thro' the superficial or cortical substance, enter the *periosteum*; and are from thence continued farther into the

 body,

body, yea even into the utmost parts thereof; we may hence reasonably conclude, that in a healthful body, as there is a constant supply of an oily substance convey'd into the bones; so this is again constantly carried out from the bones, by means of these vessels, into all parts of the body, even to the extremities of the fingers: As an evident proof of this, let any one lay the ends of his fingers upon a clean and bright pewter plate, and he shall find the pewter appear soil'd in the place where he has touch'd it; for, in reality this soil is nothing else but some oleaginous particles discharged from the ends of the fingers; there is, it is true, something of a watery substance mix'd with the oily particles; but this evaporates in a little time, and the oily particles are left behind upon the plate.

A preternatural Tumour on the Loins of an Infant attended with a cloven Spine; *by Dr.* Rutty. Phil. Transf. N° 366, p. 98.

THE accurate M. *Ruysch*, in an observation on the *spina bifida*, takes notice, that other writers have describ'd it, as cloven lengthways into two equal parts; whereas out of 10 subjects he had an opportunity of examining, not one of them prov'd in that manner, the body of the *vertebræ* being entire and the accute processes only divided.

The spine Dr. *Rutty* treats of comes near this description: It was that of a female infant six days old; whose mother, when seven weeks gone with child, upon a fright occasion'd by her husbands falling from a horse and very much bruising his loins, gave the embryo this injury; but notwithstanding, she went out her time, and the child was full grown.

There appear'd upon the region of the loins, in the same part where the father receiv'd his hurt, a tumour about the bigness of a small turnep, with a broad basis, around which the skin was discoloured by an *ecchymosis*, as it were; but from thence it immediately became pellucid, resembling a blister rais'd by *cantharides*, and continued so throughout, excepting just at its *apex*, where was a substance like a *fungus*: This vesicle was fill'd with a liquor, which in scent and colour resembled urine; insomuch, that upon strictly examining the linen stain'd by what issu'd from it, with that from the *urethra*, no sensible difference could be perceiv'd, and a communication was concluded between the left kidney and the orifice, into which

e ſurgeon's probe paſs'd obliquely upwards about an inch: Vhether the ſame ſimilitude may hold between the urine and is liquor in all caſes of the like nature, Dr. *Rutty* has not ad opportunity of obſerving; but as M. *Ruyſch* has taken o notice of it in ſuch a number of ſubjects that have been efore him, he is apt to think that it does not.

Upon opening the body, the kidneys, contrary to expecta-on, were entire, and did not any way communicate with the utward orifice.

But upon clearing away the fungous ſubſtance, which took p all the *ſulcus* or hollow of the ſpine, then it was diſcover'd here the perforation tended, a long probe eaſily paſſing up e channel, which contains the ſpinal marrow: Throughout is *fungus* were diſpers'd a great many terminations of ſmall erves, from whence diſtill'd this urinous liquor, as it were, hich occaſioned the tumour: The reſt of the *medulla* was ore compact and fill'd the cavity of the ſpine; tho' in ſome bjects it has been waſted to ſuch a degree, that by blowing to the orifice you might inflate the *dura mater*; as was done a like caſe by Mr. *Dobyns* a ſurgeon. The coat of the mour at its baſis conſiſted of the *cuticula*, *cutis* and *mem-rana adipoſa*; the two firſt of which terminating forthwith, e *cuticula* only was continued, under which immediately ap-ear'd the muſcles and fungus above mentioned: In this it iffers from thoſe of M. *Ruyſch*, which receiv'd their coats om the membranes that inveſt the *medulla*; as alſo from thoſe ken notice of by *Tulpius*, that borrow'd theirs from the *eritonæum*, as he ſays; tho' by his own deſcription, it is more kely they proceeded from the ſame membranes.

Theſe tumours conſtantly attend the *ſpina bifida*; inſomuch at when any of them preſent themſelves on the loins or back a new born infant, we have good grounds to pronounce this be the cauſe; but we may be poſitive therein, if the child can-ot move its lower limbs, the want of ſuch motion is an in-llible indication, that the ſpinal marrow reaches no farther han the ſwelling, whereby the nerves are not diſtributed to hoſe parts: They appear differently in different ſubjects; in ome the whole tumour is opaque, which is owing to ſmall laments of nerves, propagated in great numbers throughout s coat, and not from the thickneſs of the ſkin, as M. *Ruyſch* ould have it; in others it is pellucid, and then the *medulla* erminates at once at the aperture of the ſpine, and does not hoot out into any ramifications: This before us is a compound of

of both ſpecies, the greater part of which was pellucid; th
leſs, *viz.* the *apex* opaque.

The ſpine itſelf was not cleft; but had its *vertebræ*, wi
their other proceſſes entire, and was only defective in its acu
ones: But that portion of the *vertebræ*, that ſhould form a
acute angle, from whence their ſpinal proceſſes naturally ariſe
in order to form the *ſpecus* or paſſage for the marrow, gap'
and lay almoſt in a ſtreight line on each ſide; whereby th
medulla was deprived of its uſual defence from external inju
ries. This defect began at the third *vertebra* of the loins
and was continued to the end of the *Os ſacrum*; which laſt i
very extraordinary, it ſcarcely ever happening to this bone
the reaſon of which the Dr. takes to be the more than ordinary
compactneſs of its *vertebræ*, whereby it is leſs liable to be in
jured by any impreſſion: For, we obſerve that in adults they
grow ſo cloſe together, as to unite and form one large an
ſolid bone.

As the caſe before us is a *vitium conformationis*, owing t
the mother's imagination; ſo the ſame is ſometimes occaſione
by matter lodging upon the ſpine and corroding the *vertebr*
by its acrimony; but then you find a carious bone; wherea
in theſe preternatural caſes, there remain no ſuch veſtige
This was obſerv'd by Mr. *Cowper* in the body of the late Ea
of *Peterborough*; for, upon opening it, he found a larg
tumour, from which there flow'd a browniſh colour'd matter
and under it the upper and fore-part of the ninth and the
lower part of the eighth *vertebra* of the *thorax* were con
ſum'd and gone, the ſpinal marrow being cover'd with it
membranes only: This gave riſe to thoſe ſymptoms in that
morbid caſe, ſo nearly reſembling the above recited in thi
preternatural one.

It is manifeſt from what has been ſaid, that theſe caſes ar
incurable, and muſt in a little time kill the patient: But i
is almoſt immediate death to open the tumour, which every
ſurgeon will naturally do, the fluid ſeeming to require i
diſcharge.

Of the change of Colour in Grapes *and* Jeſſamine; *by* Mr Cane. Phil. Tranſ. N° 366. p. 102.

IN 1714 Mr. *Cane* planted a cutting of a vine againſt a wall
on an eaſtern proſpect; where it had the ſun from his riſing
till half an hour after 12 o'clock: The ſoil was a ſtiff clay
but to make it work the better, he meliorated it by mixing
ſome

me rubbiſh of the foundation of an old brick wall; in *Janu-ry* 1719 he prun'd it and its figure was thus,

Left hand T right hand *black*.

At time of year it ſhot at both hands about 22 inches a ſide, before it came to a joint; that on the right was very luxuriant branch, as big as the body of the tree; e other ſide was not half ſo thick or big; and the leaves on e right were as big again as the other on the left hand, and e largeſt he thinks he ever ſaw: The right hand bore a very rge and good black grape and large cluſters; the left hand ere very good white grapes; and in 1719 he had more bun-es of the white than of the black; and whereas in all vines aring black and blue grapes the leaves die red, theſe died hite on the black ſide as well as on the other: In *January* 720 he prun'd the tree again, but tack'd up more of the ght hand (being black) than he did on the left; for which aſon he had that year a great many more of the black than had of the white; and they ripen'd for the ſeaſon of the ar very well; about the 23d of *October* 1720, he gather'd e laſt of them; and the leaves died alſo white this year, eing the ſecond year of its bearing.

In *April* 1692 Mr. *Cane* having a ſmall plant of the com-on white jeſſamine, which ſtood in the ground, and was no igger than a tobacco-pipe; he cut it off at two joints above e ground, and grafted it with a cutting of the yellow-rip'd; it took and ſhot a ſmall weak ſhoot; and in a month five weeks after, it was blighted; and he perceiv'd it had ill'd the graft and ſome part of the ſtock below; ſo he took is knife and cut it to the quick, which was near the knot or int next the ground, and let it ſtand, thinking to graft it gain in ſpring, as before, but he forgot it till the ſeaſon was aſs'd: At length he obſerv'd, that it had broke out at the ext joint with ſeveral ſhoots of the yellow and green ſtrip'd; nd not only there, but alſo from the root it had a ſtrong ſhoot f yellow and green ſtrip'd: Some time after he took it up ith mold to the root; and putting it into a pot it flouriſh'd ll the ſummer: He made a preſent of it to the Preſident of *Magdalen* College *Oxon*, where it flouriſh'd two or three ears; and then for want of ſhifting the pot in time, it was matted to the bottom and ſides thereof, that it died.

Mr.

Mr. *Cane* tried ſeveral other ſorts of variegated plants; but he did not find any of them to tranſmute, as that jeſſamine will do.

An Account of ſome new electrical Experiments; *by Mr.* Stephen Gray. Phil. Tranſ. N° 366. p. 104.

MR. *Gray* having often obſerved, in the electrical experiments made with a glaſs tube and a down-feather, tied to the end of a ſmall ſtick, that after its fibres had been attracted towards the tube, when that has been withdrawn, moſt of them would be attracted to the ſtick, as if it had been an electrical body, or as if there had been ſome electricity communicated to the ſtick or feather: This put Mr *Gray* upon thinking, whether if a feather were drawn thro' his fingers, it might not produce the ſame effect, by acquiring ſome degree of electricity: This ſucceeded accordingly upon his firſt trial; the ſmall downy fibres of the feather next the quill being attracted by his fingers when held near it; and ſometimes the upper part of the feather with its ſtem would likewiſe be attracted; but not always with the ſame ſucceſs. He then tried, whether hair might not have the ſame property by taking one from his peruke, and drawing it three or four times thro' his fingers, or rather between his thumb and fore-finger; and he ſoon found that at the diſtance of ½ an inch it would be attracted to his finger; and ſoon after he found, that the fine hair of a dogs ear was ſtrongly electrical; for, upon taking the ear and drawing it thro' his fingers, great numbers of them would be attracted towards his fingers at once.

The next thing Mr. *Gray* thought of, was threads of ſilk of ſeveral colours and ſeveral fineneſſes, which he found to be all electrical; but ſometimes he could not ſucceed: The reaſon of which he afterwards found, as will appear anon.

Having ſucceeded ſo well in theſe, Mr. *Gray* proceeded to larger quantities of the ſame materials; as pieces of ribband both of coarſe and fine ſilk, and of ſeveral colours; and he found, that by taking a piece of either of theſe of about half a yard long, and by holding the end in one hand and drawing it thro' his other hand between his thumb and fingers, it would acquire an electricity; ſo that if the hand were held near the lower end of it, it would be attracted by it at the diſtance of five or ſix inches; but at ſome times the electricity would be much weaker than at others; the reaſon of which he conjectured to be, that the ribband might have imbibed ſome aqueous particles from the moiſt air, which upon trial he found to be the caſe;

ſe; for, when he had well warmed the ribband by the fire, it ver failed to be ſtrongly electrical.

After this he made trial with ſeveral other bodies, as linen of veral ſorts, *viz.* holland, muſlin, *&c.* and woollen, as of ſeve-l ſorts of cloth and other ſtuffs of the ſame materials: From eſe he proceeded to paper, both white and brown; and he und, that, after they had been well heated before rubbing, they piouſly emitted their electric effluvia: The next body, in which found the ſame property, was thin ſhavings of wood; he hi-erto only tried fir-ſhavings, which are ſtrongly electrical: The ree laſt ſubſtances he found to have the ſame property are ather, parchment, and thoſe thin guts wherein leaf-gold is aten.

All theſe bodies will not only by their electricity be attracted the hand or any other ſolid body near them; but they will, as her electrical bodies do, attract all ſmall bodies to them, and at at the diſtance of ſometimes 8 or 10 inches: Heating them the fire before rubbing very much increaſes their force.

There is another property in ſome of theſe bodies, that is mmon to glaſs, *viz.* that when they are rubbed in the dark, ere is a light follows the fingers thro' which they are drawn; is holds both in ſilk and linen, but is ſtrongeſt in pieces of hite preſſing papers, which are much the ſame with card-paper; is not only yields a light as above, but when the fingers are ld near it, there proceeds a light from them with a crackling iſe like that produced by a glaſs-tube, tho' not at ſo great a ſtance from the fingers; to perform this, the paper, before rub-ng, muſt be heated as hot as the fingers can well bear.

A down-feather being tied to the end of a fine thread of raw k, and the other end to a ſmall ſtick, which was fixed to a ot, that it might ſtand upright on the table; there was taken a ece of brown paper, which by the above-mentioned method, as made to be ſtrongly electrical; and which, being held near e feather, attracted it; and with the ſame he carried it, till it me near the perpendicular of the ſtick; then lifting up his and till the paper was got beyond the feather, the thread was xtended and ſtood upright in the air, as if it had been a piece f wire, tho' the feather was diſtant from the paper near an inch: the finger were held near the feather in this poſition, the reateſt part of the fibres next the paper would be repelled; hen at the ſame time upon holding a finger to the fibres that ere more remote from the paper, they would be attracted ereby.

Mr. *Gray* repeated this experiment without the feather, o
by a single thread of silk only about five or six inches lo
which was made to stand extended upright as above-mentio
without touching the paper; then placing his finger near the e
it would avoid or was repelled by it; but upon placing his fi
at about the same distance from a part of the thread, that
about two inches from the end, it was then attracted by it.

The several bodies found to be electrical are, 1. Feath
2. Hair. 3. Silk. 4. Linen. 5. Woollen. 6. Paper. 7. L
ther. 8. Wood. 9. Parchment. 10. Ox-guts, in which l
gold is beaten.

Some Objections, made to the Account of the Antiquity of Venereal Disease, *answered; by Mr.* Becket. Phil. Tr N° 366. p. 108.

THE two following objections were made to Mr. *Beck* account of the antiquity of the venereal disease in *P Trans.* N° 357, 365. The first is, that the venereal disease, well known among us now, and the leprosy of former ages, co not be the same disease; because the leprosy is not to be c quered by salivation, to which the other generally yields readily.

In answer to this Mr. *Becket* observes, that the leprosy have among us at this time, only affects the surface of the bo the skin generally appears scaley, with a certain deep red co or small sores upon removing the scales, and sometimes a sca ness, with a redness of the skin that affects different parts of body. Mr. *Becket* has known both the cheeks only affec both the arms for the breadth of the palm of the hand; so times the breast, the legs and other parts; but this may cont upon the patient during his life, as it frequently does, and n make any farther progress; which shews it to be a cutic disease. In these cases upon salivating the patients, the s generally fall off, the redness disappears, and the cure shall s to be compleated; but in a month or two, the same inco niencies generally attend them as before. But one ought n conclude, that because our leprosy will but rarely be cured salivation, and the pox generally will, that many of those ancients judged to be leprous were not really venereal; for, t leprosy, as they called it, was a quite different disease from o Had there been any proof adduced, that persons had been s vated in their leprosy and failed of cure, it would have de mined the case. But on the contrary we are assured by

lean

arned Dr. *Pitcairn*, in his desertation on the ingress of the *es venerea*, that the leprosy, before the *Neapolitan* disease as talked of, was cured by mercury; and now since it changed name, it is no longer heard of. Thus we find, that their prosy and our venereal disease would be cured by the same ethod; but their leprosy and ours being absolutely different seases, we by no means ought to expect the success, from the me process of cure, should be the same. None ever observed r leprosy to be attended with shedding of the hair, hoarseness the voice, speaking thro' the nose, consumption of the flesh, cers all over the body, putrefaction of the fleshy parts and of e bones themselves, filthy ulcers of the throat, corrosion and ling of the nose; all which are reckoned as symptoms of their prosy: On the contrary, ours is a mild and almost inoffensive sease, which a person may be affected with all his life long, d never become the worse: Whereas the other shewing itself the above-mentioned symptoms, brings the patient to the most serable end: Besides, their disease was got by coition, as their thors assures us; but in our leprosy, a diseased husband may habit with his wife, as long as he lives, and he shall never be le, either by coition or the immediate contact of the diseased rts with those that are sound, to communicate any malady. ad what our predecessors called the leprosy been the same sease we call by that name now, they had not been so solicitous making such large provision for them or shutting them up m human society; for, one of our leprous persons might have en among them and no body have known that he laboured unr any infirmity at all.

Hence it is evident, that the disease, so common among them, s entirely different from our leprosy, the appearances of which ar no manner of analogy with the former. It is from the sympms of the disease and the manner of its being received, that generally distinguish one disease from another; but the sympms of most of their leprous patients and the manner in which e disease was gotten, will be found in no other disease, that atks the human body, but in the venereal only; for, here they actly agree; and consequently, we cannot deny them to be the me.

The second objection is a false assertion of Dr. *Fuller*'s the storian; *viz.* that the leprosy was brought into *England* by me of our countrymen from the holy war, and that the disease as altogether unknown among us before.

In anſwer to this objection, Mr. *Becket* obſerves, that it was in 1096 the firſt *Engliſhmen* went over to the holy war, as our hiſtorians generally agree; and that ſome of them returned in 1098, two years after that expedition: But it is very certain, we had the leproſy among us before that time; for, *Wharton de Epiſcopis Londinenſibus* and other hiſtorians aſſure us, that *Hugo de Orivalle*, one of the Biſhops of *London*, died here of the leproſy in the year 1084; which proves, that our countrymen did not firſt bring that diſeaſe from the holy war: *William* of *Malmesbury*'s account of this diſeaſe is, as follows; *Is poſt paucos ordinationis annos in morbum incurabilem incidit; ſiquidem regia valetudo totum corpus ejus purulentis ulceribus occupans ad pudendum remedium tranſmiſit; nam credens aſſerentibus unicam fore ſubſidium, ſi vaſa humorum receptacula, verenda ſcilicet, exſecenter, non abnuit: Itaque & opprobrium ſpadonis tulit Epiſcopus, & nullum invenit remedium, quoad vixit Leproſus.*

Now it is highly probable, had this been a new diſeaſe the Biſhop died of, the mention of it, as ſuch, would not have eſcaped our hiſtorian: But on the contrary, it ſeems to have been anciently known among us, becauſe the remedy made uſe of for it was ſo; it having been recommended by *Ætius* and other phyſical writers ſeveral hundred years before this time; and Mr. *Becket* thinks it very plain, that the cutting off the *teſtes* and with them the veſſels deſigned for receiving the humours, as expreſſed in the former caſe, was by them looked upon to be of peculiar ſervice; becauſe it is probable, that obſerving the diſeaſe to begin in theſe and the neighbouring parts, they ſuppoſed that the very *minera morbi* would by this means be deſtroyed, and the diſeaſe either cured or its ſpreading prevented.

An Experiment to compare the Paris Weights *with the* Engliſh; *by Dr.* Deſaguliers. Phil. Tranſ. N° 366. p. 112.

DR. *Deſaguliers*, finding the accounts we have of the *French* weights different in different books, ſent for ſome *Paris* weights exactly according to the ſtandards at the *Chatelet*; and he found upon trial, the *Paris* ounce, which contains 576 of their grains, to be equal to 476 of our grains *Troy*; from which experiment all the other proportions may be deduced.

The *French* pound contains	16	ounces
Ounce	8	drachms or 576 *Paris* grains
Drachm	3	deniers
Denier	24	grains

Some Remarks on the Method of observing the differences of right Ascension and Declination by cross Hairs in a Telescope; by Dr. Halley. Phil. Transf. N° 366. p. 113.

ASTronomers are greatly endebted to M. *Cassini* for his invention of applying threads at half right angles in the common *focus* of a telescope, to determine thereby the differences of right ascension and declination of any two stars, whose situation is such, that by their diurnal motion they follow each other thro' the aperture of the telescope, fixed in such a manner, as that the first of them may pass over the center of the glass, and move exactly along one of the threads, whilst the interval of time between its transit and that of the following star, is accurately measured by a pendulum-clock, well adjusted to the mean motion of the sun or else to the revolution of the fixed stars, whereby the difference of right ascension is given; as is the difference of declination, by the time the following star takes to pass from one diagonal thread to the other: By this manner of observing Dr. *Pound* and Mr. *Bradley* did, in an opposition of the *Sun* and *Mars*, demonstrate the extreme minuteness of the *Sun*'s parallax; and that upon several repeated trials it was no more than 12″ nor less than 9″: But Dr. *Halley* considering, that in *October* 1721 *Mars* would be again in opposition to the *Sun*, about the 10th degree of *Taurus*; but not come very near any fixed star, that has a place in Mr. *Flamstead*'s *Catal.* he was solicitous to see, if there were any telescopic stars to which he would very nearly approach; and on the 28. of *February* 1720, the heavens being very serene and clear in the evening, and *Venus* having nearly the declination in which *Mars* moved *October* 1721; he fixed his telescope on her at 7^h 28′ equal time, and noted the moment she had passed over the center of his glass, or rather the common intersection of the four cross hairs; and in ½ an hour's time he observed eight very conspicuous stars, four of which, being within the compass of one degree, fell very nearly in the said way of *Mars*, and from the intervals of time he then observed, with their difference of declination from *Venus*, he determined their right ascensions and declinations, as well as her place from his tables (which by observation he found at this time needed no correction) would allow him; they all falling between the 9th and 10th degree of *Taurus* with very little Lat. But what confirmed him that all was right was, that on *March* 22. 1720, *Mercury* appearing very fair and just past his greatest elongation, he found by *Senex*'s zodiac, that he was nearly in the same parallel,

Venus

Venus had before deſcribed; and tho' the brightneſs of the *crepuſculum* effaced the ſmaller ſtars; yet in a quarter of an hour he had one paſt 10′ and ¼ more ſoutherly than the planet, which in leſs than 3′ of time was ſucceeded by another, which was but 1′ more northerly than the former; when after an interval of about 14 minutes of time, in which he was ſurpriſed to find the ſky ſo void of ſtars, the four above-mentioned ſtars paſſed ſucceſſively over his glaſs, with the ſame interval of time in which he had ſeen them follow each other on the 28. of *Feb.* whereupon he was deſirous to try, whether if the place of *Mercury* in his tables were aſſumed, the ſame right aſcenſions and declinations of thoſe ſtars would be deduced from him, as from *Venus*; and the Dr. found on trial by an exact calculation, that he had the ſame right aſcenſions now as before, in none of the four differing quite ½ a minute; ſo that thoſe ſtars may be ſecurely added to the *Catal.* and the appulſe of *Mars* to them be obſerved in very long teleſcopes in *October* 1721, to the farther aſcertaining the immenſe diſtance between the ſun and earth.

Hence it will likewiſe appear that the Dr's. mercurial numbers are at leaſt at this time, and in this part of his orb, not leſs exact than thoſe of *Venus*; and whereas this planet ſcarce ever appears with us out of the ſun's beams, and always low, and therefore, under great refraction; this way of obſerving takes off all the uncertainty that accrues therefrom; and when once the zodiac ſhall be compleated with the ſtars that are wanting to fill up the vacant places, it will be eaſy at any time by this method, to obſerve *Mercury* or a comet within the ſun's beams, with the ſame certainty, as if it were remote, and out of the neighbourhood of the horizon, where the different vapours that lie near the earth, render the appearances of the ſtars ſomewhat dubious upon account of the irregular refractions.

A Propoſal for meaſuring the Height of Places by help of Mr. Patrick's Barometer, *in which the Scale is very much enlarged; by the Same.* Phil. Tranſ. N° 366. p. 116.

SINCE *Torricelli* firſt found the mercury in an inverted tube to be *in æquilibrio* with the whole column of air over it, and the weight of the incumbent column to be various according to the different diſpoſitions of the air, in reſpect of ſerene fair weather, and of rainy, windy or otherways tempeſtuous weather; there have been ſeveral attempts and contrivances to make the minute variations thereof more ſenſible: And firſt the wheel barometer was thought of, which certainly ſhews theſe variations

with great exactneſs; but it is only proper for a fixed ſtation, and not eaſy to be removed; which circumſtance is requiſite for the principal uſe, to which this inſtrument is applicable, and for which Dr. *Halley* would recommend it.

The next thought for this purpoſe was that of Mr. *Hubin* (as deſcribed in *Phil. Tranſ.* N° 184.) who returning the tube of the barometer, as an inverted ſiphon, made a large dilatation in the aſcending leg thereof, wherein the mercury aſcended, as its altitude in the other part thereof abated, and *è contra:* Over this he drew out a narrow glaſs-tube, which he filled with tinged ſpirits, and which, being about 15 times lighter than mercury, would aſcend about 15 times as much as the mercury in the barometer fell. This, beſides that the ſpirits would dilate and contract with heat and cold, had the inconveniency of the former, of not being eaſily removed without great danger of diſordering and breaking it, by reaſon of the ſmallneſs of the tube in which the ſpirits were to riſe and fall.

This was ſucceeded by Dr. *Hook*'s marine barometer, made of two thermometers, the one, the common ſealed weather-glaſs, having no communication with the external air, wherein the temperature as to heat and cold was ſhewn by the ſwelling or ſhrinking of the included ſpirits; the other, the old thermometer, made with an inverted bolt-head, in whoſe globular part was included air ſomewhat rarer than the circumambient; ſo as to make the liquor, which was to riſe and fall in the ſhank of the bolt-head, always to ſtand above the ſurface of the ciſtern, into which its extremity was immerſed. This ſhewed the heat of the air by its dilatation, but at the ſame time the different preſſure of the atmoſphere mixed with it; ſo that the graduation of theſe two thermometers being adjuſted to any given height of the mercury, they would at all times, when the mercury was at that height, both ſhew the ſame degree of heat: But at other times, when the weight of the air was different, that difference would ſhew itſelf by the diſagreement of the degree of heat ſhewn by them. This will be better underſtood from *Phil. Tranſ.* N° 269, wherein this inſtrument is deſcribed at large. This tho' of admirable uſe at ſea, to give timely notice of approaching bad weather, labours under this objection, *viz.* that it ſuppoſes the concave of the tubes of the thermometers to be cylinders, or of equal diameters throughout; and alſo, that on account of heat and cold, the air and ſpirits have a proportional dilatation and contraction; the firſt of which the Dr. takes to be very hard to be found in ordinary

ordinary-glaſs tubes; and the other ſtill wants to be made o
by authentic experiments.

The laſt contrivance for this purpoſe is that of Mr. *Patrick*
who calls himſelf the *Torricellian* operator, by filling a ſmal
glaſs-tube about five foot long, and ſomewhat (but as little a
poſſible) tapering upwards towards the cloſe end of the tube
then inverting it without a ſtagnant ciſtern of mercury, ſo much
of the mercury as exceeds the length of the column the atmoſ-
phere can at that time ſupport, will drop off, and leave its length
equal to the then height of the common barometer: Now whe
the barometer riſes, this length in the tube becomes greater by
the mercury's being preſſed up into the upper and narrower par
of the tube; and when it falls on the contrary, it ſettles down
into the wider part thereof and becomes ſhorter, being alway
the ſame in quantity. By this means, as the angle of the con-
cave cone of glaſs, of which this tube conſiſts, is ſmaller, the
different ſituation of the mercury will, upon the alteration of the
preſſure of the air, be nicely ſhewn by very large and diſtinct
diviſions.

Now the uſe to which the Dr. would apply this contrivance of
the barometer is, to meaſure thereby the different levels of places
too remote to be come at by the ordinary inſtruments for levelling
with the certainty one would deſire. For this purpoſe let there
be provided two ſmall glaſs-tubes nearly ſimilar, tapering but very
little at the cloſe extremity; ſo that being inverted, the mercury
may be ſuſpended in them at the height it ſhould have at the
time of the experiment. Let that height be duly noted, and
then going up the *monument*, or ſome ſuch edifice where the
aſcent may be exactly meaſured, let the ſcales annexed be divided
into parts by the deſcent of the mercury, at every 10 foot, in
both the pendent barometers; which he conceives may be choſen
in ſuch a manner as to make the diviſions very diſtinct and ſenſi-
ble. Theſe being thus prepared, when it is deſired to take the
level of two diſtant places, let one of the glaſs tubes be placed
in the lower place, at a time when the mercury has the ſame
height, as when they were firſt inverted and graduated; and let
the other be carried to the higher place, where it will be found to
ſtand at that diviſion which anſwers to the height of that place
above the other, which had been before found by meaſure in go-
ing up the *monument*: Thus may 90 foot aſcent, which makes
but one tenth of an inch of mercury, be repreſented by two or
three inches, or a ſpace capable of being divided into 90 parts;
whereas,

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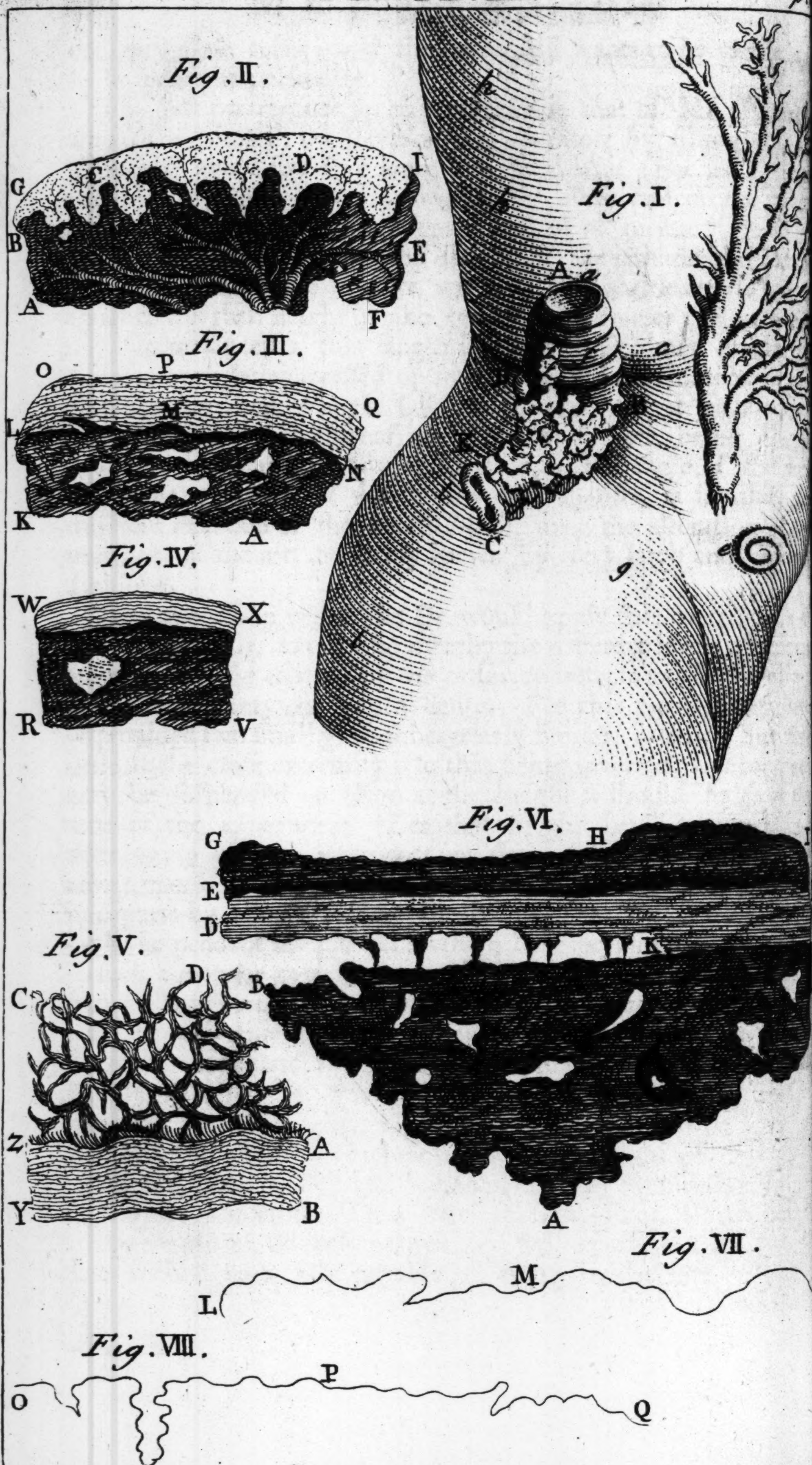

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hereas, if the diſtance of the two places be 20 miles, a minute a degree is equal to above 30 foot; and by the uſual ſights, hether teleſcope or otherways, of water-levels, the Dr. is apehenſive it will be very difficult to convey a true level without greater error than one minute in the whole.

he Variation of the Needle, *in the* Baltic; *by Mr.* William Sanderſon. Phil. Tranſ. N° 366. p. 120.

WEdneſday June 1. 1720, being at anchor near *Revel* in 58°, 58′ N. Lat. the magnetical amplitude at ſun-ſet was

weſt ———	64°	30′	N.
he true amplitude was W. ——	49	37	N.
Variation N——	14	53	W.

Saturday July 23. at the *Iſle of Gottſand* in 58° 21′ N. Lat. ſun-ſet, the magnetical amplitude was W. 49° 50′ N. and the ue amplitude W — — 35 00 N. which ves the variation N — — 14 50 W.

The difference of Long. of the two abovementioned places by *ad reck.* is 1° 50′.

October 24. At *Bornholme* in 56° Lat. at ſun-riſing, the magtical amplitude was — E. 43° 15′ S. and the true mplitude was — E. 28 31 which gives the ariation — — N. 14 44 W.

large Quantity of Alcalious Salt *produced by burning rotten Wood; by Mr.* Robie. Phil. Tranſ. N° 366. p. 121.

A White oak-tree, about two foot diameter, in *Cambridge* in *New-England*, is of ſo ſurpriſing a nature, that tho' about of it were decayed, and ſeemed to be really rotten wood; yet is decayed part would in burning turn almoſt entirely into a ood white *alkali*, and run down into hard, white and clean umps. Upon taſting of the lumps it was found to be ſalt and ry ſtrong; diſſolved in clean water, upon decantation and evaoration without filtring, a very clean white ſalt was produced xceeding in ſtrength and whiteneſs any had at the ſhops.

Now tho' *Alkali's* may be extracted from common aſhes; yet what was peculiar in this, is 1. That while it was burning, the wood itſelf would melt and run down into hard lumps of ſalt, not the ſound, but only the decay'd wood would do this; and what was moſt decay'd, would yield the greateſt quantity of ſalt. 2. Whereas all other *Alkali's* of wood, made thus by incineration, are blackiſh at firſt, and a *lixivium* made of them, tho' often filtred, will ſtill be tinged with a brown colour, occaſioned by a kind of coal or aſhes, ſo cloſely united to the *alkali* in burning, as not to be eaſily ſeparated by filtration, tho' often repeated; yet this *alkali* was very white; even before ſolution; and when diſſolv'd, the *lixivium* was not in the leaſt tinged, but clear like pure water, only a very ſmall quantity of aſhes ſubſided to the bottom of the veſſel, in which the ſolution was made. The *lixivium* thence decanted needed no filtration, but when boil'd up *ad ſiccitatem*, the ſalt remained fine and white. 3. That in the burning of this wood, as the heat of the fire grew more intenſe, the wood melted as it were and clodder'd together in large lumps; and viſibly bubbled and boil'd with an hiſſing noiſe like the frying of fat in a pan. 4. That whereas the weight of the alkalious ſalt produced from other wood, in the common way of incineration, is very inconſiderable, in proportion to the weight of the wood that produces it; yet this ſalt nearly equalled in weight the wood from which it was taken. 5. Whereas the aſhes of other wood are never ſo replete with ſalt, as that it can be obſerv'd, or in the leaſt cauſe the aſhes to lump or clod together; yet the whole of this would gather into hard and ſolid lumps of white ſalt, as eaſily to be diſtinguiſhed from aſhes, tho' white, as the pureſt ſalt of tartar, made with nitre, would be. 6. That tho' a much leſs quantity of an *alkali* can be produced from other rotten wood than from ſound wood; yet here it is quite the contrary, the decay'd part of this tree yielding a large quantity as aforeſaid; and the other or ſound part yielding no more than other wood.

Mr. *Robie* having examined the tree and conſider'd what (by the marks found thereon) probably happened to it, firſt premiſes, that in all likelihood it was ſtruck with lightning ſeveral years before; it being torn from the top of the trunk to the bottom on that ſide which was decay'd, and yielded the aforeſaid ſalt, there being a channel from top to bottom about five inches wide, as he ſuppoſes, at firſt, and which length of

of time had clos'd up; and under this bark, the wood next to it was black, which he ſuppoſes, was caus'd by lightning.

Hence he conjectured, that the wood being thus expoſed to the air and water for ſo long a time, was the occaſion of its becoming defective in that part; and that the lightning having penetrated the wood, had ſo alter'd and diſpos'd its parts and pores (the figure and texture of the parts appearing very different from other rotten wood) to attract, imbibe and retain the nitrous ſalt of the air, which in ſo long a time, muſt have been very copious: Even as ſalt of tartar or other *alkali's*, expos'd to the air for ſome conſiderable time will be entirely reduced to a nitrous ſalt, as *Glauber* affirms, and its quantity likewiſe conſiderably increas'd, not that the lightning had calcin'd the wood in ſuch a manner, as to reduce it to a perfect ſalt; yet by penetrating into it, it had calcin'd it ſo, as to impart to it a like property or diſpoſition of attracting the nitrous ſalt of the air, as the aforeſaid *alkali's* of wood that have been perfectly calcined.

Now if it ſhould be objected, that the nitre in this wood being volatile would fly away in the burning the wood: To this Mr. *Robie* anſwers, that tho' nitre cannot be fix'd and reduced to an alkalious ſalt, by calcining it *per ſe*; yet it may, by the addition of the powder of charcoal, as chemiſts tell us; and here he ſuppoſes the wood ſo alter'd by lightning in which this nitrous ſalt was lodg'd, as to ſerve inſtead of coal in the burning of it.

An Account of a Foetus *that continued 46 Years in the Mother's Body; by Dr.* Steigerthal. Phil. Tranſ. N° 367. p. 126.

ANNA *Mullern* of the vilage of *Leinzelle* near *Gemund* in *Suabia*, of a dry and thin conſtitution, but otherways healthful and robuſt, died at the age of 94, after ſhe had liv'd a widow 40 years.

Six and forty years before her death, ſhe declar'd herſelf to be with child, and ſhe had all the uſual ſigns of pregnancy: At the end of her reckoning the waters came away, and ſhe was taken with the pains of labour, which continued upon her about ſeven weeks, and then went off upon uſing ſome medicines given her by a ſurgeon. Some time after, ſhe recover'd her perfect health, only that her belly continued ſwell'd, and that now and then upon any exerciſe, ſhe felt ſome pain in the lower part thereof: She was after this twice brought to bed;

the firſt time, of a ſon, and afterwards of a daughter: But notwithſtanding this, ſhe was firmly perſuaded, that ſhe was not yet deliver'd of what ſhe firſt had gone with; and ſhe deſir'd Dr. *Wohnlixe* the Phyſician of *Gemund*, and one *Knauſſen*, a ſurgeon at *Heubach*, to open her body after her death: Accordingly dying on the 11th of *March* 1720 after four days illneſs, her body was opened by the ſurgeon, who found within her a hard maſs of the form and ſize of a large ninepin bowl, without obſerving whether it lay in the *uterus* or out of it.

A Fig. 1. Plate IV. repreſents a part of the integument, which adher'd to a ſpongy fleſhy ſubſtance; this at firſt ſeem'd to be a maſs of cartilage, but was afterwards found to be entirely bony; B B B C C C ſhews the membranous part, which was bloody; D D D D the opening made by the hatchet, which was the inſtrument made uſe of; E E E E E another part of the integument appearing entirely bony, with ſeveral prominencies; F a contuſion occaſioned by the manner of opening it, where there appeared ſome putrified membranes.

A A A Fig. 2. repreſents the integument or ſubſtance incloſing the *fœtus*; B the *fœtus*; C a depreſſion or hollowneſs on the right cheek; α the noſe turning up; β the mouth flatted, but not ſo wide, as here repreſented; γ the eye clos'd up; δ the ear; D D the arms, of which the right was the larger, and the two joints of the right thumb were plainly to be ſeen; E the protuberance of the knee; F part of the *funiculus umbilicalis* torn, but ſtill adhering to the navel; G G part of the ſame *funiculus* adhering to the bony part of the integument; H the breaſt; I the mark of an inciſion into the left ſide, where the fleſh appeared red, but was dry and look'd like ſmoak'd beef.

Fig. 3. repreſents two ribs from the left ſide, of their natural ſubſtance, colour and bigneſs; A A the part joining to the *vertebræ*; B B the part joining to the *ſternum*.

This is preſerv'd in the cabinet of rarities of the Duke of *Wirtemberg*.

Dr. *Camerarius*, Profeſſor at *Tubingen*, in a letter on this ſubject, takes notice, that the ſurgeon found this maſs in a cavity on the woman's left ſide, and that it adher'd to the membranes of that cavity by the intervention of a ſpongy fleſhy ſubſtance: From which particular, and the woman's having had two children, during the time that this large maſs lay in her, that learned Profeſſor conjectures, that it was not lodged in the womb, but in the left *tuba Falloppiana*, which by

y this means, had been very much dilated and thicken'd in s ſubſtance.

bſervations on the Membranes, encloſing the Faſciculi *of Fibres, into which a Muſcle is divided; by M.* Leewenhoeck. Phil. Tranſ. N° 367. p. 129.

M. *Leewenhoeck*, in cutting off ſeveral thin ſlices from a piece of beef, whenever he cut the fleſhy fibres thro' ranſverſely, could plainly diſcover the membrane (as it is ommonly call'd) which runs between and envelopes the leſhy fibres, and eſpecially the larger *faſciculi* of them, as hey run lengthways along the muſcle: Between theſe *faſciculi* he membrane is of a conſiderable thickneſs, but ſpreads out every way into exceeding ſmall ramifications. Some weeks before, he obſerv'd, that this membrane was compos'd of an inconceivable number of very minute veſſels, which were plainly o be diſcern'd, not only where the membrane appear'd of ſome onſiderable breadth, but even where it was not ſo broad as a ingle muſcular fibre; but how far this held, he could not determine, foraſmuch as theſe ſmall ramifications of the membrane did again ſpread themſelves into other ramifications ſo exceeding fine, eſpecially where they inclos'd the ſingle muſcular fibres, that they were in a manner inviſible, even thro' his beſt microſcopes.

The very ſmall veſſels compoſing this membrane (as it is call'd) are doubtleſs deſign'd to convey ſome nutritious juices: Yet they are ſo ſmall, that the globules of blood cannot paſs thro' them.

ABCD Fig. 4. Plate IV. repreſents a ſmall piece of the membrane, which, with the adjoining fleſhy parts, is cut thro' tranſverſely; and it being impoſſible to draw the vaſt number of veſſels, which compoſe it, on account of their exceeding minuteneſs, they are repreſented only by points; EFG and HI repreſent the carnous fibres cut thro' tranſverſely along with the membranes.

Theſe carnous fibres, when moiſt, lay ſo cloſe to each other, that the ſpace between EFG and HI was quite fill'd up; but when dry, they were ſo ſhrunk up, that one might obſerve ſuch intermediate ſpaces, as in the figure.

Now as we ſee, upon the drying of the membranes AFG and DEG, with the muſcular fibres between them, what number of ſmall ramifications proceed from the membranes, as is repreſented between the muſcular fibres; we muſt not imagine

gine, that theſe ramifications proceed only from the points here repreſented; but that they are continued the whole length of the fibres; and ſubdividing themſelves into ſtill finer ramifications, they incloſe every ſingle fibre in the whole muſcle.

Among ſeveral pieces of fleſh, where the carnous fibres were cut tranſverſely, M. *Leewenhoeck* happened on one piece, with its branches ſo plain, that the membranes and fibres reſembled ſo many boughs of trees with the leaves on, as repreſented by K L M N Fig. 5. where M ſhews the membrane torn off from another, as alſo the number of branches it runs into, and the ſeveral fibres it covers.

All theſe carnous fibres, with their membranes, lay very compact together, when he cut them off from the pieces of fleſh; as alſo when he laid them upon the glaſs and moiſtened them; but as the moiſture evaporated, they ſhrunk again, in the manner here repreſented; and tho' the deſigner could plainly diſtinguiſh the ſmall veſſels cut thro', the largeſt of which appeared at M, yet he was obliged to mark them with points only. Here you may obſerve, that all the carnous fibres, being cloſely tied together by the ſaid membranes, by which they were enveloped, and which are nothing but a congeries of veſſels, could not be ſeparated from each other upon drying, but by tearing thoſe membranes aſunder.

The carnous fibres together with the membrane at K L M N Fig. 5. do not take up ſo much room, but that a grain of ſand may cover it; and yet in ſome of thoſe carnous fibres, one might very diſtinctly obſerve, the parts of which they were compos'd.

He purſu'd this obſervation in the fleſh of a whale, of which he had two pieces by him, for about ſeven or eight years, of a ſpan long and two inches thick; from theſe he cut ſeveral ſlices tranſverſely, but found the carnous fibres, ſo cut thro', eaſily ſeparate from each other; ſo that he could not find his account in this, but thought the membrane was rotten; he therefore cut off the outſide with a table-knife, and then with a very ſharp knife he cut the inner part into very thin ſlices; and there he found the excrements of mites, very ſmall, but globular; and ſome of them as ſmall as ever he had obſerv'd; and theſe excrements he found every where, eſpecially where the membranes were thickeſt; then viewing the parts where the membranes were thinneſt, he there diſcovered the aforeſaid veſſels, in as great number as he had obſerv'd them in an ox's fleſh, and as diſtinctly.

After

After the former discoveries as to the circulation of the blood; particularly, that the blood-vessels have no terminations, he began to consider how the fat particles could be form'd; since he did not think, they were separated from the blood and emitted from the blood-vessels: But having now plainly discovered, that these membranes were nothing but very small vessels, and supposing that they were form'd for no other end, but to convey nutriment; as also that there was no circulation in these vessels, he imagined, that the matter call'd fat, was brought into them; which, when there was too great a supply of nutriment, so as that it could not be farther propelled, must be driven out of these vessels; for, all the particles of fat he hitherto observ'd, were inclos'd in small films.

This original of the fat seems more probable, than that it should be forced out of the blood-vessels; and yet how these fatty particles are form'd, which consist of small globules, and these of still smaller globules, he could not hitherto determine; as also where these vessels, which constitute these membranes, have their origin and how this fat is convey'd into them.

M. *Leewenhoeck* had in his drawer a piece of ox flesh, that had lain there about four years, wrapp'd up in a paper; and he found this piece of flesh in some places covered with a membrane; from this he cut off several small slices along with the membrane; and he found, that there lay near the membrane about 16 or 18 nervous fibrils, which, in the drying of the flesh were so squeez'd together, that they were almost twice as long as broad; in some of which he observ'd very distinctly those vessels, which are in the nerves.

These nervous fibrils were inclos'd in a sort of half-round, which separated them from the muscular fibres, and which consisted of a row of small tendinous fibrils, each of which was about twice as thick as the hair of a man's beard; without these tendinous fibrils by the muscular fibres, which had been cut thro' transversely; and in this part of the half-round there were several apertures, which seem'd in the microscope to be big enough for a grain of hemp-seed to pass thro', and might well be taken for vessels, but that there lay so many of them together: But considering, that the nerves are commonly cover'd with fatty particles, he concluded, that these apertures were no vessels, but meer fatty particles, which he found to be so upon cutting thro' them; and discovered that the inner fat was eaten out by the mites, which had left only the husks

or

or *cortices* of the fat globules behind; which *cortices* he could never hitherto discover; because they would, upon any heat, melt away as fast as the inner fat itself.

Observations on the Vessels in several Sorts of Wood *and on the muscular* Fibres *of different* Animals; *by the Same.* Phil. Transf. N° 367. p. 134.

M. *Leewenhoeck*, having procured a piece of wood (brought from the island of *Amboyna* in the *East-Indies*, and of which cabinets are made) sawed off at the end of a board, as also some of the chips in order to observe the vessels therein; and cutting the wood all manner of ways, he found, that in one place it appeared whitish, at a small distance red, and in another place blackish. Upon cutting it transversely, he observed the orifices of the ascending vessels, which ran along the length of the wood, and which appeared of such a size in the microscope, that one would have judged a pea might pass thro': Where the wood looked reddish, he found these large vessels filled with a substance of a fine red colour; so that he imagined, these large vessels conveyed a red sap into the horizontal vessels, which appeared so very numerous and so thick together, that they caused the wood to seem of the same colour with the red substance, contained in them.

He afterwards cut off some very thin slices transversely from this wood, and putting them into a china-cup, he pour'd some hot water upon them and suffered them to lie therein for some time; then viewing them with a microscope he observed, that the red substance was extracted by the water, and no red colour was now to be found in any of the vessels.

What seemed most surprising to him in this wood was, that upon cutting thro' it lengthways, as he frequently did, he observed it to be of a fine red colour for a hair's breadth; and a hair's breadth farther it appeared white; and the ascending vessels seemed to be smaller, where the wood was red than where it was white; which narrowness of the red vessels he judged to be owing to the sap contained in them.

In viewing the ascending vessels in oak, he found some other vessels, which entered into their sides and appeared to him like so many small round holes; especially, where the horizontal vessels lay, which he judged to be united to the ascending vessels, by means of those small orifices, and thereby to discharge part of their sap into them.

M. Lee-

M. *Leewenhoeck* taking a ſmall twig of an oak, which in ſeven years growth was about the thickneſs of one's finger, cut it thro' lengthways both in the aſcending and horizontal veſſels, which laſt he obſerved lying in great numbers very cloſe together, and iſſuing directly from the pith of the twig.

He likewiſe made ſome obſervations on fir-wood, in which the aſcending veſſels conſiſt of ſo very fine and thin a ſubſtance, as to exhibit a very agreeable ſight in the microſcope: In theſe aſcending veſſels he imagined, he ſaw ſome globules, with a ſmall opening in their middle, which ſeemed to be of a cloſer and denſer ſubſtance than the reſt of the wood; but afterwards he found himſelf miſtaken, and that theſe ſuppoſed globules were nothing other than the orifices, by which the aſcending and horizontal veſſels were united together, and thro' which the ſap was conveyed from the one to the other.

From theſe obſervations he turned his thoughts to the fleſhy fibres of animals, and began to conſider with himſelf, that ſince the author of nature uſually obſerves the ſame frame and ſtructure in a great variety of his creatures; perhaps, the fine membranes, with which every muſcular fibre is inveſted, and which are provided with a vaſt number of ſmall veſſels, might convey nouriſhment in the ſame manner, thro' every carnous fibre in a healthful body.

In this view he cut off ſome very ſmall thin ſlices from the fleſh of an ox, directly acroſs the length of the fibres, and placing them upon glaſſes, and moiſtening them with clean rain-water, he obſerved them with a very good microſcope, and continued viewing them ſo long, till the fleſhy fibres began to grow dry: He then obſerved, that in ſome places the exceeding ſmall and fine veſſels, which compoſed the membranes that incloſe the fleſhy fibres, were broken off from the ſaid fibres, by the unequal ſhrinking of the thin ſlice of fleſh upon the plate of the microſcope. At the ſame time he obſerved ſome other of theſe ſmall veſſels, which were ſomewhat ſtronger than the former, and were not broken off from the fleſhy fibres; but yet were ſtretched and drawn from them to the diſtance of the diameter of a blood globule. He likewiſe obſerved ſome fleſhy fibres, which adhered ſo cloſe to other fibres, that the ſmall veſſels of communication were not broken off or ſtretched; ſo that nothing was to be ſeen there, but only the membrane encompaſſing the fibres.

He likewiſe placed before the ſame microſcope ſeveral other fleſhy fibres, which he had ſeparated lengthways from the fleſh

of an ox: In each of theſe he obſerved a great many ſmall apertures, by which he judged, that the ſmall veſſels of the membranes had entered the fibres; and moiſtening them with water, no ſooner was the little moiſture, left in thoſe apertures, evaporated then he could ſee them very plain and diſtinct.

In order to examine into the truth of this hypotheſis, M. *Leewenhoeck* pricked his thumb with a fine needle, and placed a little blood upon the glaſs, where the fleſhy fibres lay, in order to obſerve with his microſcope the proportion between the diameters of a globule of blood and of the aforeſaid apertures, which he had obſerved in a fibre. And it was found that the diameter of a blood-globule was four times as large as that of one of theſe apertures; and if ſo, then according to the known rule, a globule of blood muſt be divided into 64 parts, before it can enter one of theſe apertures in a fleſhy fibre.

This diſcovery ſeemed very ſurpriſing to M. *Leewenhoeck*; and he is apt to think, that it will be very difficult to penetrate any farther into the hidden ſtructure of the muſcular fibres, and the manner in which they receive their nouriſhment.

M. *Leewenhoeck* having in a former *Tranſaction* aſſerted, that the muſcular fibres are compoſed of long ſmall filaments, he was ſollicitous to diſcover whether theſe ſmall filaments, which compoſe a carnous fibre, might not really be ſo many ſmall veſſels: With this deſign he took part of the fleſh of a whale, he had kept ſome years by him; and cutting it into very thin ſlices directly acroſs the fibres, and having moiſtened theſe thin ſlices with fair water, he placed them upon ſeveral glaſſes, and before ſeveral microſcopes; when he obſerved, that what he had formerly taken for ſmall threads or filaments, were in reality exceeding ſmall veſſels. He then cut part of the whales's fleſh lengthways, in order to diſcover the veſſels, which convey the nutritious juice out of the membranous into the muſcular fibres; which veſſels then appeared to him very numerous and very diſtinct.

He afterwards took another piece of the fleſh of a very fat ox, which he cut thro' tranſverſely, and looking upon it with ſome of his beſt microſcopes, he could plainly ſee, that how ſmall ſoever theſe fibres were, they were ſtill vaſcular; for, he could ſee the light thro' the apertures of theſe veſſels, as he had done before in thoſe of a whale; but if he happened to cut the fibres never ſo little obliquely, inſtead of cutting directly

directly acroſs their length, the light was not to be ſeen thro' them.

He had in a drawer the hinder quarter of a mouſe, that had lain there ſome years; from the largeſt muſcle of which he cut off tranſverſely ſome ſmall ſlices, as thin as poſſible: Then placing theſe before his microſcope, he not only obſerved, that the carnous fibres were of the ſame thickneſs with thoſe of an ox; but beſides, he could obſerve the apertures of the veſſels, compoſing the carnous fibres, as plainly as in the fleſh of a whale: The veſſels in the muſcular fibres of a whale are, it is true, ſix times more in number than in thoſe of an ox or a mouſe, but then the fibre of a whale is alſo ſix times as thick as the other.

Hereupon he conſidered after what manner the veſſels, of which the muſcular fibres moſtly conſiſt, received their nouriſhment from the veſſels of the membranes; ſince the muſcular fibres when at reſt have ſeveral alternate corrugations, by which he judged, that the veſſels in the fibres muſt have their ſides preſſed together and their cavities cloſed up: But if we recollect, that in walking a man may move both his feet upwards of 3600 times in an hour (for, he may make two ſteps in the time of one pulſation of the artery) and that in that ſpace of an hour the muſcular fibres muſt be ſo many times dilated and contracted, and will therefore, require conſiderable ſupplies; we ſhall likewiſe find, that this is ſufficiently provided for, ſince upon every extenſion of the muſcle, the apertures of thoſe ſmall veſſels are free and open for the conveyance of nouriſhment into the fibres.

Experiments relating to the reſiſtance of Fluids; *by Dr.* Deſaguliers. Phil. Tranſ. N° 367. p. 142.

ON the 30. of *March*, 1721. Dr. *Deſaguliers* took a ball of gold an inch in diameter, that had a ſmall ſtem of the ſame metal, with a place thereon to faſten a ſtring to; and having ſuſpended it by a ſilken thread, too ſtrong to lengthen by ſtretching; he made the diſtance between the center of the ball and the point of ſuſpenſion, equal to 12,5 inches; then cauſing the ball to vibrate in a trough full of water (which had an upright piece of wood in the middle of one ſide, with pins or keys from which the ball hung, that the center of ſuſpenſion might always be in the ſame place) he obſerved by looking from a pin on one ſide of the trough to a mark made oppoſite to it on the other ſide, whereabouts the ſtring of the

pendulum (juſt above the ſurface of the water, in which the ball was quite immerſed) went after 14 vibrations; and by another pin and oppoſite mark, he alſo obſerved where it went to, after 28 vibrations. Taking out the water, he filled the trough with mercury; the length of the pendulum, the point of ſuſpenſion and all other things remaining as before; then letting go the ball in the mercury from the ſame place whence it was let down, when the trough was full of water (which was marked by a ſtring ſtretched acroſs to prevent miſtakes) after one entire vibration, it came very little ſhort of the ſame mark, as it came to in water, after 14 vibrations; and when it vibrated twice in mercury, it came to the ſame place it had done after between 26 and 28 vibrations in water; and thus it did exactly ſeveral times.

Afterwards filling with mercury an upright copper pipe of four inches diameter to the height of three foot 10 inches, and ſuſpending the golden ball therein by a ſhort ſtring about an inch long; ſo as to have the ball juſt immerſed under the middle of the ſurface of the mercury; he cauſed it to be let down ſuddenly, and obſerving how long it was falling down to the bottom of the tube, he found, that the experiment was diſturbed by the ball's ſtriking againſt the ſides of the tube, which retarded the fall of the ball, and the more ſo, the oftener the ball ſtruck. When the ball was leaſt retarded, it was only two ſeconds and ½ in falling, which muſt be taken as the true time of the fall of the ball in a height of quickſilver, equal to 3 foot 10 inches; becauſe when he tried the experiment again at home the 1. of *April* following, the ball fell in the mercury once or twice without ſtriking the ſides of the tube at all, but not in leſs time than two ſeconds and ½.

He alſo repeated the other experiments at home, making the golden pendulum 39,2 inches long, ſo as to make it vibrate but once in a ſecond; and then he found, that it would vibrate five or ſix times in the mercury, before the vibrations became ſo ſmall as not to be obſerved; and then the firſt vibration in the mercury ended very near where the 14th in water had done; the ſecond in mercury ended where the 27th in water had done; and obſerving the third vibration in mercury, it ended exactly at the mark, where the 40th in water ended; and this was obſerved by ſeveral perſons beſides.

Then the Dr. weighed 14 penny-weight of the mercury (in which he made the experiments) firſt in the air, then in water, where it loſt only one penny-weight and one grain of its weight;

weight; that is, it weighed in air 336 grains and in water 311; so that its specific gravity was to that of water as 13,44 to 1.

As to the golden ball that had varnish and cement upon it to keep the mercury from sinking into it, he found it weigh as follows;

	oz.	dwt.	gr.
In mercury	1	00	18 or 498 grains
In water	5	01	00 or 2424 gr.
In air	5	07	09 or 2577 gr.

Dr. *Desaguliers* took the wire and pendulum of a long pendulum-clock; and having fasten'd the golden ball at the end of the wire under the pendulous weight that served for the clock, in order to make the vibrations of the golden ball in the mercury continue longer; he did not find it keep on its motion above a swing or two the longer for it; nor did a round ball of lead, placed upon the said wire, just above the surface of the mercury avail any more; and as he found some inconveniencies in these two last ways of making the experiment he rather chose to rely upon those made with the golden ball suspended by a silken thread of 39,2 inches long, measuring from the point of suspension to the center of the ball.

An Account of the Poison-wood-tree *in* New-England; *by Mr.* Dudley. Phil. Transf. N° 367. p. 145.

THE poison-wood-tree grows only in swamps or low wet grounds, and is somewhat like a small ash, but much more like a *sumach*; and, therefore, is by some called the *swamp-sumach*; for, its twigs, leaves and shape are exactly like the *sumach*; and it likewise bears a dry berry.

It never grows bigger than a man's leg, nor taller than alder; but spreads much, and there are several together, especially about the stump or roots of one that is cut down; as it is of a quick growth, so it does not last long; the inside of the wood is yellow and very full of juice, as glutinous as honey or turpentine; the wood itself has a very strong unsavory smell, but the juice stinks as bad as carrion.

As to its poisonous quality; it must be observed 1. That it poisons two ways, either by the touch or by the smell; the scent of it, when cut down in the woods or on the fire, being poisonous to a very great degree: One of Mr. *Dudley*'s neighbours was blind for above a week together, with only handling it; and a Gentle-

Gentleman in the country, ſitting by his fire ſide in winter, was ſwelled for ſeveral days with the ſmoke or flame of ſome poiſon-wood, that was in the fire. 2. It has this effect only on ſome particular perſons and conſtitutions; for, Mr. *Dudley* has ſeen his own brother not only handle but chew it without any harm at all; and ſo by the ſame fire one ſhall be poiſoned and another not at all affected. 3. This ſort of poiſon is never mortal, and will go off in a few days of itſelf, like the ſting of a bee; but generally the patient applies plaintain-water, or ſallet-oil and cream. 4. As to its operation; within a few hours after the perſon is poiſoned, he feels an itching pain that provokes a ſcratching, which is followed by an inflammation and ſwelling; ſometimes a man's legs only have been poiſoned and run with water.

Mr. *Dudley*'s neighbour that was ſo badly poiſoned with handling it, told him one thing very remarkable of the wood, and that is, that when he touched it, he plainly perceived it to differ from the other wood he was throwing up into his cart; for, it was as cold as a piece of ice; and he withal aſſured him, he could diſtinguiſh it blindfold, or in the dark from any other wood in the world, by its coldneſs: He farther told Mr. *Dudley*, that he felt an itching in a few hours after he had handled the wood, but that the ſwelling did not come on till about three days after.

A farther Account of the ſame Tree; *by Dr.* William Sherard. Phil. Tranſ. N° 367. p. 147.

THE account Dr. *Sherard* had of the poiſon-tree from Mr. *More* (which probably he had from Mr. *Dudley*) is as follows.

The poiſon-tree grows to the bigneſs of an alder, the wood is as cold as ice; when laid on the fire, at which five or ſix perſons ſit, ſome will fall a ſwooning, fainting or yawning for ſome days; others only for a few hours; and others of the company are not at all affected: Mr. *More* could handle, cut and burn it without any hurt; and ſo it is, with ſeveral others, he ſuppoſes, according to their ſeveral conſtitutions: It was never known to kill any body, only to do hurt to ſome perſons.

Mr. *More* calls it a water-ſhrub: It is a ſpecies of *toxicodendron*, tho' not mentioned by Dr. *Tournefort* in his *Inſtitutions p.* 610; but Dr. *Sherard* takes it to be *arbor Americana alatis foliis, ſucco lacteo venenata. Pluknet. Almag.* 45. *Tab.* 145. Fig. 1. which is a ſpecies of *toxicodendron*, that grew formerly at *Chelſea* garden. What makes Dr. *Sherard* take it to be this is,

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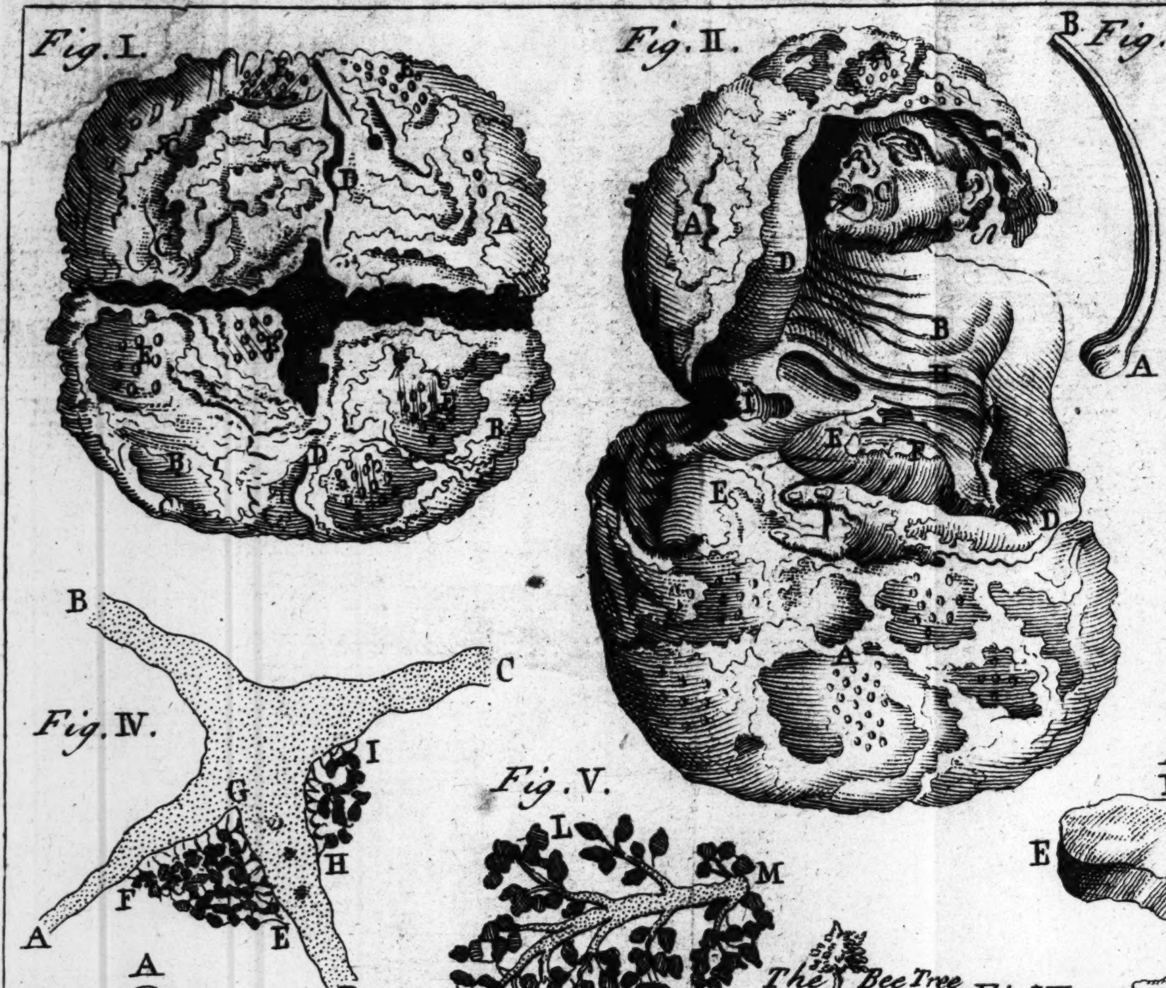
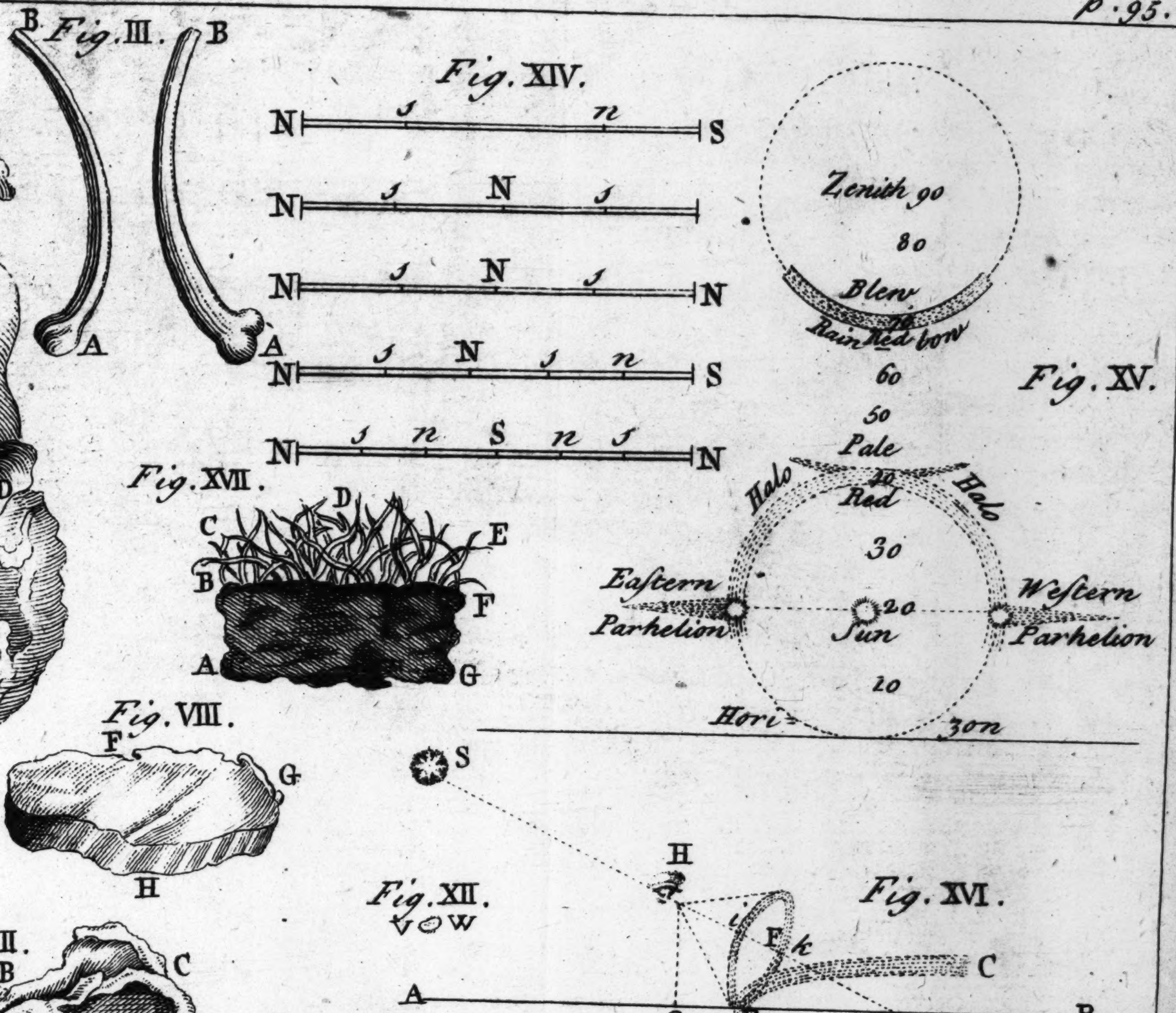


Fig. I.
Fig. II.
Fig.
Fig. IV.
Fig. V.
Fig. VI.
Fig. VII.
Fig. X
Fig. XIX.
Fig. XX.
Fig. XXI.
The Bee Tree
First Bee
The Second Course
First Station
Second Station
The Hunter

Smith del.

ATE. IV.

p. 95.



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s, Mr. *Dudley*'s saying it is like a *sumach*, and that it is by some alled the *swamp-sumach*; this, in its manner of growing and lated leaves, very much resembles the *sumach* or *rhus*; the uit is a white roundish dry berry, growing in clusters; and so, ike that of *toxicodendron triphyllon folio sinuato, pubescente, Inst. R. herb.* 611. *Hederæ trifoliæ Canadensi affinis planta: Arbor venenata quorundam H. R.* Paris; as scarce to be distinguished from it.

An Account of a new Method in New-England *for discovering where the* Bees Hive *in the Woods, in order to get their* Honey; *by Mr.* Dudley. Phil. Transf. N° 367. p. 148.

THE hunter in a clear sun-shiny day takes a plate or trencher with a little sugar, honey or molosses, spread thereon; and when got into the woods, he sets it down on a rock or stump: This the bees soon find out; for, it is generally supposed, that a bee will smell honey or wax at above a mile's distance: In a box or other conveniency the hunter secures one or more of the bees, as they fill themselves; and after a little time, he lets one of them go (for, when one goes home from the sugar-plate, he returns with a considerable number from the hive) observing very carefully the course the bee steers; for, after he rises in the air, he flies directly on upon a strait course to the tree where the hive is.

For this purpose the hunter carries with him his pocket-compass, his rule and other implements with a sheet of paper; and sets down the course, supposing it west, or any other point, and by this he is sure the tree must be somewhere in a west line from where he is; but he wants to know the exact distance from his station: In order to determine that, he makes an offset either south or north (suppose north) an hundred perches or rods (if it be more, it will still be more exact; because the angle will not be so acute) then he lets go another bee, observing his course also very carefully; for, this bee being loaded, will, as the first (after he is mounted a convenient height) fly directly to the hive; this second course (as it must be called) the hunter finds to be south, 54 degrees west; then there remains nothing but to find out, where the two courses intersect or which is the same thing, the distance from B to A or from C to A, as in Fig. 6. Plate IV. for, there the honey-tree is.

For

For which reason, if the course of the second bee from C had been south west and by south, *viz.* to D; then the hive-tree must have been there; for, there the lines are found to intersect.

All this is founded on the strait or direct motion of bees, when bound home with their honey; and this is found to be certain by the observation and experience of the hunters every year; especially, since this mathematical way of finding honey in the woods has been used with such success.

All the bees they have in their gardens or woods, and which now are in great numbers, are the produce of such as were brought in hives from *England*, near a hundred years ago, and not the natural produce of this part of *America*; for, the first planters in *New-England* never observed a bee in the woods, till many years after the country was settled: But what proves it beyond dispute is, that the *Aborigines* (the *Indians*) have no word in their language for a bee, as they have for all animals whatsoever proper to, or aboriginally of the country; and, therefore, for many years they called a bee by the name of *Englishman's* fly.

They formerly used to find out honey in the woods, by surprising and following one bee after another by the eye, till at length they found out where the bees hived.

It is observable, that when bees swarm, they never go to the northward, but to the southward, or to a point inclining that way.

An Account of the Moose-deer *in* America; *by the Same.* Phil. Trans. N° 368. p. 165.

THE *moose* is an animal, not only proper, but thought peculiar to *North America* and one of the noblest creatures of the forest: The *Aborigines* have given him the name of *moose*; *Moosûk* in the plural.

There are two sorts of *moose*; the common light *grey moose* and the large or black *moose*: The former the *Indians* call *Wampoose*; they are more like the ordinary deer, spring like them and herd sometimes to the number of 30 together.

The black *moose* is chief of the deer kind; has several things in common with other deer; differs in several things, but in all very superior to them: The *moose* is shaped much like a deer, parts the hoof, chews the cudd, has no gall, his ears large and erect: The hair of the black *moose* is a dark grey; as also the ridge of his back to 10 and 12 inches long, of which the *Indians* make

make good belts; he has a very ſhort bob-tail: Mr. *Neal* in his hiſtory of this country, ſpeaking of the *mooſe*, ſays they have a long tail; but he is miſtaken as to his account of other things beſides the *mooſe*.

The hunters have found a buck or *ſtagg-mooſe*, 14 ſpans high from the withers, reckoning nine inches to a ſpan; a quarter of his veniſon weighed upwards of 200 pounds: A doe or hind of the black *mooſe* of the fourth year was killed within two miles of *Boſton*, which from the noſe to the tail meaſured between 10 and 11 feet; and wanted an inch of ſeven foot in height.

The horns of the *mooſe*, when full grown, are between four and five foot from the head to the tip, with ſeven ſhoots or branches to each horn, and they generally ſpread about ſix foot: When the horns come out of the head, they are round like an ox's horns; about a foot from the head, they begin to grow a palm broad and further up ſtill broader, of which the *Indians* make good ladles that will hold a pint: When a *mooſe* goes thro' a thicket or under the boughs of trees, he lays his horns back on his neck, not only that he may make his way the eaſier; but to cover his body from the bruiſe or ſcratch of the wood: Theſe huge horns are ſhed every year: The *doe-mooſe* has none of theſe horns.

A *mooſe* does not ſpring, or riſe in going, as an ordinary deer, but ſhoves along ſideways, throwing out his feet, much like a horſe in a racing pace: One of theſe large black *mooſe* has, in his common walk, been ſeen to ſtep over a gate or fence five foot high. After you unharbour a *mooſe*, he will run a courſe of 20 or 30 miles, before he turns about or comes to a bay; when they are chaſed, they generally take to the water; the common deer, for a ſhort ſpace, are ſwifter than a *mooſe*; but then a *mooſe* ſoon outwinds a deer.

The fleſh of a *mooſe* is excellent food; and tho' it be not ſo delicate as common veniſon, yet it is more ſubſtantial and will bear ſalting: The noſe is reckoned a great dainty; Mr. *Dudley* eat ſeveral of them and found them to be perfect marrow: The *Indians* told him, they could travel three times as far after a meal of *mooſe*, as after any other fleſh of the foreſt.

The *black mooſe* are not very gregarious, not above four or five being found together; the young ones keep with the dam a full year.

A *mooſe* calves every year and generally brings two: The *mooſe* bring forth their young ones ſtanding, and the young fall

 from

from the dam upon their feet: The time of their bringing forth is generally in *April*. *Job*. cap. 39. v. 1, 2, 3. *Canst thou mark when the hinds calve, or knowest thou the time when they bring forth? They bow themselves, they bring forth their young ones, they cast forth their sorrows.*

The *moose* being very tall and having short necks, do not graze on the ground, as the common deer, neat cattle, &c. do; and if at any time they eat grass, it is the top of that which grows very high or on steep rising ground. In the summer they feed on plants, herbs and young shrubs, that grow on the land; but mostly and with greatest delight on water-plants; especially a sort of wild colts-foot and lilly, that abound in the ponds and by the sides of the rivers; and for which the *moose* will wade far and deep; and by the noise they make in the water, the hunters often discover them: In the winter they live on browse, or the tops of bushes and young trees; and being very tall and strong, they will bend down a tree as big as a man's leg; and where the browse fails them, they will eat off the bark of some sorts of trees, as high as they can reach: They generally feed in the night, and lye still in the day.

The skin of the *moose*, when well dress'd, makes excellent buff; the *Indians* make their snow-shoes of them: Their way of dressing it, which is reckoned very good, is this; after they have hair'd and grain'd the hide, they make a lather of the *moose*'s brains in warm water; and after they have soak'd the hide for some time, they stretch and supple it.

Some Remarks on the Allowances to be made in Astronomical Observations for the Refraction of the Air, *together with an accurate Table of Refractions; by Dr.* Halley. Phil. Transf. N° 368. p. 169.

WERE the medium of our air much more in quantity or the force of gravity much greater than it is; or in a word, were the refractive power of the air much more sensible than we find it, nothing could have been a greater impediment to discoveries in astronomy: For, all objects appearing by refraction higher than they really are, till such time as the laws and quantity of that refraction had been ascertain'd, it would have been impossible to have been secure of the true place of any celestial object. But as it falls out to be so little, that none but nice instruments can perceive its effects, it was not

not discover'd to be any at all, till *Bernard Walther*'s time, about the year 1500; nor brought to any sort of rule till *Tycho Brahe*; nor ascertained, till Sir *Isaac Newton* made the first accurate tables of it. The curve which a beam of light describes, as it approaches the earth, is one of the most perplext and intricate that can well be propos'd, as Dr. *Brook Taylor* has shewn in the last proposition of his *Methodus incrementorum*.

By this table it follows, that the ratio of the sine of the angle of incidence to that of the refracted angle, increasing as the beam approaches, makes a very notable difference in the true place of an object near the horizon; but in objects that are much elevated, the refractions become small, and their differences scarce exceed a second in each degree; so that they are much the same, as if the incident and refracted angles were on the surface of a sphere of air of the same uniform density, close adjoining to the eye.

When, therefore, the stars are 20 degrees or upwards elevated above the horizon, we may take it for granted, without sensible error, that the sines of the true and apparent distances from the vertex, are in the same constant ratio: Hence it will appear, that the distances of all the stars are seen less than they really are, in whatever position they are taken; and that no less than a second in each degree of the distance; that is, a distance of 30 degrees, for example, is contracted at least so many seconds; and one of 60 degrees no less than a minute, if the distances be taken by an instrument, truly divided: So that when *Hevelius*, to shew the exactness of his observations, brings eight distances, as taken by his sextant, which exactly compleat the circle, both in Long. and R. Ascens. the consequence is really quite opposite to his design; for, if those distances were the true ones, they being all contracted by appearing thro' a refracting medium, the sum of the eight differences both of Long. and R. Ascens. ought to fall short of a whole circle or 360 degrees by at least six minutes: So that Dr. *Halley* is apt to think, that the 60 degrees of *Hevelius*'s sextant wanted about a minute of its true quantity.

Such an allowance as this may, perhaps, be a proper expedient, to avoid accounting for refraction in celestial observations, provided the objects be nearly parallel to the horizon, or at a good height above it: For, all distances of stars, when parallel to the horizon, by the same constant quantity are con-

tracted by refraction, (be they high or low) that is, by about one second in each degree; the chords of the arches of the real and visible distances being always in the same ratio as is the sine of the angle of incidence to that of the refracted angle.

And this is the case wherein the refraction of the air does least affect the distances of the stars; which distances are still more and more contracted, as they are nearer to a perpendicular situation: So that a distance, for example, of 30 degrees loses but half a minute in a horizontal site; but if the one star be 20 degrees high, and the other 50, it will be lessened by above three times as much; or by 1 minute 41 seconds: If the one be 30 and the other 60 degrees high, the same distance will appear less than 30 degrees by about one minute; the difference still decreasing, as the objects are more elevated above the horizon: But in all cases to account for the effect of the refraction on the distances of the stars, requires, besides some trigonometrical calculation, the help of the above-mentioned table, hereto subjoin'd, and such as Dr. *Halley* had it from its great author, Sir *Isaac Newton*.

A Table

Table of the Refractions of the stars for their apparent altitudes.

app. alti. deg.	min	refraction m.	sec.	app. altit. degr.	refraction m.	sec.	app. altit. degr.	refraction. m.	sec.
0	0	33	45	16	3	4	46	0	52
0	15	30	24	17	2	53	47	0	50
0	30	27	35	18	2	43	48	0	48
0	45	25	11	19	2	34	49	0	47
1	0	23	7	20	2	26	50	0	45
1	15	21	20	21	2	18	51	0	44
1	30	19	46	22	2	11	52	0	42
1	45	18	22	23	2	5	53	0	40
2	0	17	8	24	1	59	54	0	39
2	30	15	2	25	1	54	55	0	38
3	0	13	20	26	1	49	56	0	36
3	30	11	57	27	1	44	57	0	35
4	0	10	48	28	1	40	58	0	34
4	30	9	50	29	1	36	59	0	32
5	0	9	2	30	1	32	60	0	31
5	30	8	21	31	1	28	61	0	30
6	0	7	45	32	1	25	62	0	28
6	30	7	14	33	1	22	63	0	27
7	0	6	47	34	1	19	64	0	26
7	30	6	22	35	1	16	65	0	25
8	0	6	0	36	1	13	66	0	24
8	30	5	40	37	1	11	67	0	23
9	0	5	22	38	1	8	68	0	22
9	30	5	6	39	1	6	69	0	21
10	0	4	52	40	1	4	70	0	20
11	0	4	27	41	1	2	71	0	19
12	0	4	5	42	1	0	72	0	18
13	0	3	47	43	0	58	73	0	17
14	0	3	31	44	0	56	74	0	16
15	0	3	17	45	0	54	75	0	15

The Variation of the Magnetical Compass *in the* Pacifick Ocean; *by Capt.* Rogers; *with Remarks thereon; by Dr.* Halley. Phil. Transf. N° 368. p. 173.

IN 1709-10 Capt. *Woods Rogers*, having travers'd the *Great South-sea* or *Pacifick Ocean*, set down the variations of the magnetical compass in his passage from *Cape St. Lucar* in *Calefornia* to the island of *Guam* or *Guana*, one of the *Ladrones*, being about seven hours or 105 degrees of Long. This might have been long since expected from Capt. *Dampier*, who had three several times made the tour of the world, and thrice gone this very same track.

It were to be wish'd, that the *French*, who have had frequent opportunities of doing it, would give us an account of the variations they have found in their voyages from *Peru* and *Chili* to *China*; and that the *Spaniards* would tell us, how the needle varies at this time, *viz.* 1721 in the north part of that great sea, thro' which they return from the *Manilla*'s to *New Spain*: With these helps, having three points in each curve, we might be enabled with tolerable certainty to compleat the system of the magnetical variations, which Dr. *Halley* was oblig'd to leave unfinish'd, as to this part of the ocean, in his general chart thereof, for want of the requisite observations.

The following account is an extract from Capt. *Roger's* journal; wherein the first column gives the correct Lat. of the place; the second the Long. west from *London*, as estimated by reckoning; and the third the variation, which in this whole track is easterly.

Variations observ'd in the Great South-Sea, from the South Cape of Calefornia *to the Island of* Guana *or* Guam, *one of the* Ladrones.

January 17$\frac{09}{10}$	Lat. N. cor. every Day.		Long. West from *London.*		Variation Easterly.	
12	22	16	114	09	03	00
	21	18	114	42	02	50
	20	24	115	15	02	50
15	19	25	115	45	02	50
	18	56	116	24	02	45
	18	00	117	06	02	45
			117	30	02	15

Variations observ'd in the South-Sea,

January 1709-10,	Lat. N. correct. daily		Long. West from *London*.		Variation Easterly.	
15	16	32	118	05	02	00
20	15	44	118	54	01	50
	15	00	120	15	01	30
22	14	49	122	05	01	10
	14	36	124	25	00	50
	14	24	126	45	00	40
25	14	14	129	05	00	45
	13	50	131	23	00	50
	13	29	132	58	01	00
	13	29	134	41	01	10
	13	22	136	48	01	15
30	13	27	139	21	01	25
	13	32	142	07	01	30
Feb. 1	13	32	144	37	01	40
	13	36	147	32	01	50
	13	26	150	18	02	00
5	13	26	153	02	02	10
	13	26	155	19	02	25
	13	26	157	43	02	30
	13	25	160	31	02	50
	13	41	163	00	03	00
	13	41	165	18	03	20
10	13	44	167	26	03	30
	13	36	169	56	03	45
	13	33	172	27	04	00
	13	36	175	00	04	30
	13	32	177	21	05	20
15	13	40	179	28	06	30
	13	47	181	24	07	00
	13	54	183	22	07	30
	13	52	185	37	09	00
	13	40	187	42	10	15
20	13	28	189	49	11	00
	13	21	191	30	11	30
	13	12	193	25	12	00
	13	07	194	37	11	50
	13	10	195	51	11	00
25	13	03	197	51	10	00
	13	00	199	03	09	50
	12	57	200	16	09	30
	12	54	202	20	09	00
March 1	12	58	204	12	08	40
	13	04	206	06	08	20

Vari-

Variations observ'd in the South-Sea.

1709-10.	Lat. N. correct. daily		Long. West from *London*.		Variation Easterly.	
March 3	13	05	207	33	08	00
	13	05	209	04	07	50
5	13	02	211	54	07	30
	13	97	212	42	07	10
	13	07	214	07	07	00
	13	03	215	08	06	50
	13	08	217	11	06	30
10	13	16	218	27	05	40
Island of *Guana* in sight.						

By this it appears; that at about 250 or 300 leagues west from the south cape of *Calefornia*, the east variation diminishes to about ¾ of a degree; that for 1300 leagues from thence, the same easterly variation gradually increases to about 12 degrees, where it becomes greatest; and that at the isle of *Guam*, 500 leagues still more westerly, it is again decreas'd to five degrees 40 minutes.

As far as this single instance can direct us, the Dr. is apt to think, that in all that space of sea, lying to the northward of the track, between *Japon* and *Calefornia*, there prevails an easterly variation, which is still greater and greater, as the N. Lat. increases: But that to the southward of the track and especially to the southward of the equinoctial, a westerly variation arises, of no great extent or quantity, but which is greatest about 1000 leagues west from the coasts of *Peru* and *Chili*, about the same meridians where Capt. *Rogers* found the east variation smallest.

This is agreeable to the theory of the variation laid down by the Dr. in *Phil. Trans.* N° 148; and in his seventh remark on the observations there cited, he expressly mentions that, undoubtedly there was such a track of west variation in the southern parts of the south-sea; it being the necessary consequence of the site of the four magnetical poles, there suppos'd; tho' at that time he wanted experiments to prove it.

As far

A farther Account of the Art of Diving *or living under* Water; *by the Same.* Phil. Transf. N° 368. p. 177.

IN *Phil. Transf.* N° 349. Dr. *Halley* sufficiently explained the method he had practised and found effectual to furnish air at ny reasonable depth under water, and in any quantity desired, r the subsistence of men, that shall have occasion to work on recks or otherwise at the bottom, under a great pressure of water. This the Dr. did by means of the diving-bell, which being om time to time replenished with fresh air, he had found suffient to maintain five men for near two hours together in 10 fahom water, without the least hurt or inconveniency. But the ell being not to be moved from place to place, but by moving e vessel from which it was suspended, was a great impediment the work that was to be done below; and therefore, the Dr. ethought himself how to enable the diver to go out from the ell to a considerable distance and to stay a sufficient time without , with full freedom to act as occasion served: And considering, at the pressure being greater on the surface of the water in the ell, than on any other surface that was higher than it, the air ould by a pipe pass from the bell into a cavity of air over that igher surface; he concluded, that putting on a cap of lead ade weighty enough to sink empty, and in shape resembling e bell itself, he might, by flexible pipes, which a man might arry coiled on his arm, receive a constant stream of air from e magazine thereof in the large bell, so long as the surface of e water in the caps was above the level of that in the bell.

In pursuance of this, the Dr. procured pipes to be made, hich answered all that was expected from them: They were seured against the pressure of the water by a spiral brass wire, hich kept them open from end to end, the diameter of the caity being about the sixth part of an inch: These wires were oated with thin glove-leather, curiously sewed on and then the ather was dipped into a mixture of oil and bees-wax hot, which lling up its pores, made it impenetrable to water; then several lds of sheeps guts were drawn over them, which when dry, had good coat of paint; and then the whole was secured with anoer coat of leather to keep them from fretting: The pipes (of hich several were made were about 40 foot long, the size of a alf inch rope; the one end thereof being fixt in the bell, at ome height above the water, and the other end fastened to a ock, which opened into the cap; the use of the cock being to op the return of the air, whenever there was occasion to stoop

down, or go below the ſurface of the air in the bell, which was neceſſary, as often as there was occaſion of going out or returning into the bell.

The diver, therefore, putting on his cap, and coiling his pipe, (like a rope) on his arm, as ſoon as he is diſcharged from the bell, opens his cock and walks on the bottom of the ſea, veering out the coils of his pipe, that ſerve as a clue to direct him back again; and this the Dr. has ſeen practiſed, without any bad conſequence attending it.

But there are two things to be remarked in this affair; firſt, that the weight of a man being very little more than that of his bulk in water, he cannot act with any ſtrength, nor ſtand with any firmneſs; eſpecially, where any thing of a ſtream runs, without a conſiderable addition of weight; and, therefore, the leaden caps were made to weigh about half a hundred weight, to which he added a girdle of large weights of lead (of about the ſame weight in the whole) to be wore about the waiſt; and two clogs of lead for the feet, of about 12 pounds each: With this acceſſion of weight the Dr. found a man could ſtand well in an ordinary ſtream and even go againſt it. The other thing neceſſary to be provided againſt was the cold of the water, which tho' it could not be entirely taken off, ſo that a man could endure it long, yet it was much abated by waiſtcoats and drawers, made cloſe to the body, of that thick ſort of woollen ſtuff whereof blankets are made: This being full of water, would be a little warmed by the heat of the body, and keep off the chill of new cold water coming on it.

As to ſeeing under water; as long as the water is not turbid, things are ſeen ſufficiently diſtinct; but a ſmall degree of thickneſs makes perfect night, in no great depth of water.

In the leaden caps, which from their uſe the Dr. called *caps of maintenance*, he at firſt fixed a plain glaſs before the ſight; but he ſoon found, that the vapour of the breath would form ſuch a dew on the ſurface of the glaſs, that it hindered its tranſparency. To remedy which, he found it neceſſary to prolong that ſide of the cap that was before the eyes, and thereby enlarge the proſpect of what was beneath.

An Account of an Aurora Borealis, *obſerved at* Dublin. Phil. Tranſ. N° 368. p. 180.

AT *Dublin Feb.* 6. 1720-1 the obſervations made on an *aurora borealis* are, as follows; the air was all that day, as it had been for ſome time before, very clear and ſharp; about half an

an hour past four in the evening, some flying clouds appeared, and the sky was tinged with a very unusual yellowish colour, which, perhaps, might be reflected from a large quantity of snow, that soon after fell for near a quarter of an hour: However that might be, the author dates the beginning of the ensuing phænomena from the first appearance of this uncommon light: About a quarter past six, a thin vapour, which was hitherto ill defined, and in all appearance resembled an exceeding black cloud, had fixed itself in the northern hemisphere; its edges were tinged with a reddish yellow, that by degrees, as it approached the vertex, became more diluted, till at last it ended in a faint whiteness: That it really was no cloud, but only an exceeding pure and limpid vapour, was manifest, because several of the fixed stars shone thro' it, without having their light in any degree effaced: In the middle of this dark basis, about ½ an hour past six, a lucid area shewed itself due N. E. about 35 degrees above the horizon; and in less than a minute from the time he first discovered it, emitted a very large pyramidal stream of shining vapour, which with an incredible swiftness ascended obliquely towards S. S. W; so as to leave the zenith considerably to the westward; and very soon after, about the same place, six others arose at the same instant almost up to the zenith. From this time till 48 minutes past six, there were repeated projections of these lucid rays, without any order as to time, place or magnitude. They did not only arise from behind the dark basis, but sometimes out of the pure sky, as it were; and tho' some of them continued visible upwards of a minute, yet the greater part of them only just shewed themselves and died away. The author had now got to the top of a convenient observatory, where, tho' destitute of instruments, he had a free prospect of the horizon and in company with another Gentleman, fixed himself with great attention, to expect the ensuing phases of this phænomenon.

About six hours 55 minutes between N. W. by N. and W. N. N. there was the representation of a very bright *crepusculum*, such as that, which appears about 20 minutes after sun-set; and from it arose several very large beams of light, not exactly erect towards the vertex, but somewhat declining to the south: Among these one, which arose about N. W. and in three or four seconds of time passed over 50 or 60 degrees of a great circle, was the most splendid of all that had preceeded; its sides were inclined to each other in an angle of about 8 or 10 degrees and tinged with a brisk lively red, which by degrees, as it approached the

 axis,

axis, became more intenſe and dirty: On the other hand receding from the axis, its colour was a pale yellow, that ſoon loſt itſelf in a faint whiteneſs.

From this time not a moment paſſed without ſuch variety of different phaſes, that it was impoſſible for the eye of any ſingle perſon to purſue it thro' the ſuddenneſs of its alteration. While ſome of the lucid beams ſeemed to ſtand fixed, as it were, among the ſtars, others moved ſlowly from eaſt to weſt, by which they ſeemed to meet each other, ſometimes to recede from each other; and ſometimes by a kind of appoſition, large ones were produced from others of an inferior order.

The lucid area, which the author firſt diſcovered in the N. E. had now formed itſelf into a parallelogram, whoſe upper and lower edges were five or ſix degrees diſtant from each other, and nearly parallel to the horizon; in this, as if behind a curtain, vaſt waves of light, whoſe extremities did not reach the periphery of the dark baſis, ſeemed to meet and pervade each other; at other times, while ſome of them, with a remarkable velocity, moved eaſtwards; others, as if behind them, would fly towards the weſt; by which variety of different motions, as often as any interval paſs'd between the colliſion of theſe erect waves a beautiful undulation was produced; and its pulſes, by the adjoining parts of the fluid, were propagated to a vaſt diſtance.

While he ſtood amazed at this ſurpriſing ſight, the axis of the coloured pyramid, which aroſe in the N. W. had moved conſiderably towards the weſt, and at ſeven hours 25 minutes was about 23 or 25 degrees to the north of *Venus*. The dark baſis of this meteor had now extended almoſt to the eaſt point of the horizon; and at ½ an hour paſt 7, between E. N. E. and E. by N. ſeveral large columns aſcended in an inſtant to the zenith; the moſt eaſtward of which was remarkably convex towards the ſouth, and tinged with a pale red, as were moſt of thoſe that aſcended with it: They were met by others, that aroſe at the ſame time between the north and weſt, and in the zenith formed a vaſt collection of vapour, that pretty much reſembled ſmoke, enlightened by the ſun's beams; its waves reflected a briſk, lively red colour and in ſome places a pale yellow; they rolled indifferently every way; and in little more than a minute, when the firſt efforts of their congreſs were ſpent, and all ſeemed fixed and ſerene, the *corona* projected ſeveral ſmall rays, which, with a ſlow uniform velocity, deſcended between W. by N. and N. W. ſoon after which it died away.

At

At 7 hours 40 minutes ſeveral other *ſtriæ* were emitted from hind the dark baſis, which interſecting with others, that at e ſame time aroſe about the eaſt and weſt points, formed in the nith, or rather 6 or 8 degrees to the ſouth thereof, a ſecond uch more elegant and ſurpriſing than the former, and, indeed, an any thing had hitherto appeared; it was tinged not only ith different orders of red and yellow, but alſo with blue and iolet; the laſt of which by a mixture with the white light, ppeared faint and inclined to purple. Tho' the vapour of which is and the preceeding *corona* were formed, was ſo exceeding in and pure, that ſeveral of the fixed ſtars were viſible thro' ; yet it reflected a light ſo copious, that he could thereby per-ctly diſtinguiſh the time of night by a ſmall watch: All this hile, the phænomenon ejected four or five large columns to-ards the N. W. (beſides others towards the ſouth) which ap-eared pointed at the top, and their ſides inclined to each other n an angle of five or ſix degrees: When their baſes were ex-nded about 30 or 35 degrees from the vertex, the lower parts f two or three of them broke, by the mere weight of the va-our (as it were) ſeparated from the upper and deſcended with a ow motion, in the form of truncated cones; they were gradu-lly formed by their upper parts, and in about a minute's time oſt in a large body of light, that was ſettled between the N. W. y N. and W. N. W. The *corona*, as if exhauſted by theſe reat diſcharges, became immediately more dilute and languid; s lively colours faded and were ſucceeded by a whitiſh vibrating ight, that in leſs than two minutes entirely vaniſhed.

The dark vapour, which continued to poſſeſs the polar regions, ad now extended itſelf from the E. to the N. W. by N. point f the horizon, and was formed into a large ſegment of a circle, whoſe center was about 20 degrees below the horizon; its upper dge was tinged with a pale red, which was ſoon loſt in a florid yellow; and this again, as it approached the zenith, became more effoete and languid: In this dark ſegment ſeveral lucid area's diſcovered themſelves, with a vibrating light, which in-ſtantly diſappeared, as if a curtain were drawn over them; and from it rays of very different magnitudes continued to aſcend without any uniformity as to time and place, till 48 or 49 min. after 7; when a third *corona*, very little, if at all, inferior to the preceeding ones, either in the variety of its colours, or the quan-tity of light it emitted, was formed in the zenith. As both the preceeding were produced by the northern *ſtriæ*; ſo this was augmented by two or three large ones, that aroſe due ſouth, out

of

of the pure ſky, and were, in all probability, part of the vapour which had been projected beyond the zenith, or which had ſubſided from the two former: They cauſed the vapour of which this image was compoſed, to move with great violence, in different directions, not unlike waves of ſmoke, confined in a reverberating furnace; this motion being abated, the vapour acquired a kind of ſtagnation, in which ſtate it continued but a very ſhort time, before it projected ſeveral lucid beams (a certain fore-runner of its approaching diſſolution) between the N. and W. and ſoon after it vaniſhed.

About this time the large beam, which aroſe in the N. W. and had preſerved its colours in their original beauty, for more than ¾ of an hour, began to fade, and at $7^h\ 53'$ was abſorbed in a vaſt body of light, which ſeemed fixed in that part of the horizon; it had moved in that time 15 or 20 degrees to the weſtward of the place from whence it aroſe: The *impetus* of the vapour being now pretty much abated, there was nothing extraordinary, but ſucceſſive diſcharges of pointed rays between the N. W. and E. N. E. without any order or uniformity as to time or place; ſetting aſide theſe, there was very little difference in the general face of things for 20 minutes; neither was there much reaſon to hope for any, becauſe the ſeverity of the cold was ſuch as obliged the ſpectators to retire and ſo loſe the enſuing phaſes of the dying meteor.

An Account of an Aurora Borealis, *obſerved on the ſame Day at* Cruwys-Morehard *in* Devonſhire, *as alſo of the Weather both before and after this* Phænomenon; *by Mr.* Cruwys. Phil. Tranſ. N° 368. p. 186.

ON *Monday Feb.* 6. 1720-21 a little before ſeven in the evening, there aroſe out of the north or a little towards the eaſt, a bright *crepuſculum*, which ſoon ſpread itſelf a great way thro' the northern part of the hemiſphere. About ſeven (when Mr. *Cruwys* firſt obſerved it) it began to leave behind it at due north or a few degrees to the eaſt, part of a very clear ſky (which looked like a black cloud, but the ſtars ſhone clearly thro' it) being a ſegment of a circle, into which figure the *crepuſculum* or expanded body of lucid vapour had now likewiſe formed its upper limb, making a kind of broad *iris*, terminated at each end by the horizon.

All this while the ſtreaming lights appeared in great variety, as to figure, place, magnitude and colour; but for the moſt part of a redder colour, eſpecially towards the weſt, than the *crepuſ-*

ulum itſelf, out of which they ſeemed to be formed; and tho' he greateſt appearances had been for the moſt part within 20 egrees of the north on each ſide, yet at due N. W. there were a reat many conſiderable ones.

About eight this *crepuſculum* (which had been conſtantly, tho' owly, carried farther from the north) had, with the upper part f its outer limb, reached to about 10 or 12 degrees beyond the ole-ſtar towards the zenith, being now upwards of 30 degrees road, with a circular ſegment of black clear ſky to the north, of bout 25 degrees, when the whole *crepuſculum* or vapour was all f a ſudden formed into aggregate bodies like vapours, and was ne of the moſt agreeable ſights, that had been obſerved of the ind: The baſes of the cones ſeemed to reſt on the upper limb f the ſegment of clear ſky (which was extended near 60 degrees n each ſide the N.) and the vertices of the cones, pointing all owards the zenith, approached within a few degrees of it and erminating there, formed the greateſt part of a ſemicircle, incloſed, s it were, with golden palliſadoes, which ſhining all at once lmoſt as bright as flame, and being of a vaſt length and number, xhibited a very agreeable ſight.

This laſt phænomenon ſhewed, that theſe cones were collections f the very ſame particles, of which the *crepuſculum* had con-ſiſted; becauſe when it appeared every where alike and equal, he great ſtars ſhined thro' it but very faintly: Whereas after-wards, thoſe ſtars, that remained between the cones, ſuddenly ppeared very bright; whilſt thoſe, that were covered by them, ould hardly be perceived: And, indeed, all the ſtreaming lights his evening ſeemed to flow from this *crepuſculum* downwards, as rom a fountain or ſtore, and not to ariſe from the horizon; few pproaching it nearer than 10 degrees, and many not within 20 r 30 degrees.

After this fine appearance had continued about two minutes, he matter ſeemed to be exhauſted and the ſcene almoſt at an end, he ſtreaming ſhining lights being moſtly extinguiſhed and the emaining parts of vapour, reſembling broken clouds; when the laſhing lights began to appear of a moſt prodigious ſwiftneſs, oth from N. E. and N. W. pointing to the zenith or a little more to the ſouth: It was obſervable, that over the tracts where heſe flaſhing lights paſſed, the remaining parts of vapour (that ow lay ſcattered every where, like white broken clouds) pointed r ſeemed to have a tendency, conformable to the ſame motion: Whereas towards the due north, where no flaſhes appeared, theſe whitiſh clouds lay confuſed and irregular as before.

This

This continued about 20 or 25 minutes, when the wind began to arise a little at N. E. and the scene was quite at an end; dark clouds succeeding all over the N. and by 9 o'clock there was a severe storm of snow.

N. B. That all the time of these appearances, several broken parts of the extinguished vapours, resembling white clouds, were carried beyond the zenith; some 50 or 60 degrees, and others even to the horizon itself at S. S. W.

During the whole continuance, there seemed to be a small, easy breath of wind, scarce perceivable, at N. N. E. which the motion of the clouds above-mentioned also confirms; but as soon as it began to blow a little brisker, the remaining parts of the vapour were all dissipated.

January 30. 1720-1. Hard snow in the morning and frost all night.

January 31. and *February* 1, 2, 3, 4. Pleasant sun-shine days, very calm, but the snow still lying and at nights very hard frosts.

Feb. 5. Very violent snow in the morning, some thaw in the afternoon and hard frost at night.

6. Hard frost in the morning and the wind exceeding cold and sharp, but not hard; the afternoon pleasant, sun-shine and calm; but it froze all day out of the sun; and continued to do so all the evening, and the ground was still covered with snow.

7. Very hard frost in the morning, and frost and storms of snow all day; the wind at N. N. W.

8, 9, 10, 11. Pleasant calm days; but hard frost and very hard frost at night.

12. Hard frost; exceeding cold wind at S. E. at night these lights were said to have appeared again; as also on the 18. to a very great degree, but Mr. *Cruwys* did not see them; the weather still frosty with some snow.

Feb. 22. At night hard snow.

23, 27. A thaw and some rain and but little frost afterwards, only dry cold winds, till the 27. when the frost returned very sharp, with exceeding cold winds at N. E. and S. E. for a fortnight and upwards, without any snow, and did a great deal of damage.

bſervations on the Muſcular Fibres *of* Fiſh; *by M.* Leewenhoeck. Phil. Tranſ. N° 368. p. 190.

THE aſſertion, that nature in all her various productions conſtantly obſerves the ſame courſe and manner of operan ſeems contrary to M. *Leewenhoeck*'s obſervations; either oſe made on the generation of animals and the ſeeds of plants; even thoſe on the muſcles and muſcular fibres of different anials; foraſmuch as the muſcles of fiſhes are not provided with y tendons: On this occaſion M. *Leewenhoeck* examined anew e muſcles of the cod-fiſh, on which he made the following obrvations.

After the late diſcoveries he had made of the ſmall veſſels in e muſcular fibres of a whale, ox, ſheep and mouſe, he was t to imagine, that the ſame fabric would likewiſe hold in the uſcular fibres of fiſh: But as this could not be certainly conuded, having at that time a part of a freſh cod, he cut off a iece of the fiſh, on purpoſe to examine it with his microſcope me days afterwards.

He cut this piece of fiſh into ſmall ſlices, ſome according to e length of the fibres and others directly a-croſs, in order to bſerve, whether theſe muſcular fibres were compoſed of great mbers of ſmall veſſels, running along the fibre: And in effect e found, that when he had cut the fibres thro', there appeared the microſcope as great a number of ſmall veſſels running ong theſe fibres, as he had formerly obſerved in the muſcular bres of a whale.

But what appeared moſt remarkable was, that in a great numr of fibres, in which he could not diſcover any veſſels running ngthways, he obſerved a vaſt many ſmall veſſels, which ſeem'd proceed from the membranes encompaſſing the fibres: For, in e fibre theſe veſſels appeared to proceed from the circumference circular tunicle of the fibre and to paſs on to the oppoſite part ereof: And in another fibre cut tranſverſely, he obſerved veſſels iſing from the circumference and dividing themſelves into aller branches about the middle of the fibre; all which, as r as he could perceive, terminated again in the circumference of e fibre: In one fibre he obſerved at leaſt fifty of theſe veſſels, nning thro' each other.

Upon this diſcovery he found, that he had been miſtaken what he had firſt imagined, to wit, that the veſſels which oſe from the membranes, proceeded no farther than juſt thro'

 the

the tunicle of the fibre, and so discharged the fluid into the fibre for its nourishment: Whereas now he perceived, that the vessels which arose from the membrane and enter'd into the fibre, did not end there, but spread themselves into smaller branches, proceeding every way from the inside to the tunicle of the fibre. This made M. *Leewenhoeck* imagine, that the nutritious juice might circulate in these small vessels, just as the blood does in the veins and arteries; and that what the muscular fibres receiv'd from them, might be no more than what ouz'd thro' the tunicles of these small vessels, as in land-animals, which have no other termination than the artery proceeding from the heart and the vein terminating therein; the artery and vein thus forming one continued vessel,

Having now a great many fibres lying before him, in which he could observe very plainly the vessels just now treated of, yet he could not discern in the transverse sections of the fibres any appearance of those vessels, which run along their length and compose the greatest part of the body of each fibre: this he imputed to the cutting of those vessels not directly across, but somewhat obliquely, by which their apertures had been clos'd in such a manner, that he could not perceive them, nor the least resemblance of them.

M. *Leewenhoeck* several times observ'd between the muscular fibres of the fish, a great many vessels lying together, which compos'd what is call'd a membrane; which vessels surrounded the muscular fibres, and lay so many of them together, that the thickness of the whole *fasciculus* of vessels was equal to that of a muscular fibre, and as he imagin'd, was afterwards to be dispers'd in smaller ramifications between the fibres.

In viewing an entire muscle of a cod-fish, and the fibres of which it was compos'd, he found the thick end of the muscle equal to the back of an ordinary knife, and the thinner not to exceed the thickness of a single fibre: Several of these fibres are twice as long as the thickness of the muscle; and between the muscles lie what are commonly call'd membranes, which are nothing else but a congeries of vessels. These vessels do not only run between the fibres, but into the very substance of every fibre, as we observe, when the fibres are cut transversely: By these vessels the muscular fibres and the entire muscles themselves are so firmly bound together, that they serve instead of tendons to each other.

In like manner the muscular fibres are united to the bones y the vessels proceeding from them, which vessels compose what in land animals is call'd the *periosteum*.

ABCD Fig. 7. Plate IV. represents two muscles of a od-fish, lying close together, as they are united to each ther and separated from the other muscles; the part repre-ented by A B C having been cover'd with the skin near the ead of the fish; and M. *Leewenhoeck* supposes, that the ody of the cod-fish, from head to tail, consists of a continu-d series of such muscles.

EFGH Fig. 8. represents a single muscle of the fish; where E H G shews the thickness of the muscle; and its thin dge, which is no thicker than the edge of a knife is mark'd y E F G.

When these muscles had lain several days upon a paper, hey were not dried so hard, but that he could split them into hin shivers, one of which is represented between the letters and K Fig. 9. in order to shew the oblique course of the ibres, which are represented by small lines.

M. *Leewenhoeck* turn'd his thoughts to river fish and particu-arly to pearch; and as he imagin'd, that an old pearch had o greater number of muscular fibres than a young one, but nly that the fibres increas'd in bigness during the growth of he fish; and that the larger these fibres were, the more lain and distinct must be the small vessels, of which the ibres were compos'd, he procured one that weighed three ounds and half, and was 17 inches and $\frac{1}{4}$ long *Delft* measure, which is the same with the *Rhinland*.

He cut off four pieces from this fish, *viz.* two from the ack near the head, and two others from the belly in the thick art of the fish, on purpose to make his observations thereon ext day.

Accordingly at that time he view'd the muscular fibres oth as to length and breadth, and found, that the fibres of his large pearch were not so thick as those of the cod-fish: Upon cutting them thro' lengthways, he observ'd the aper-ures of the small vessels in very great numbers: He next cut ome of the fibres transversely, and he plainly found them hinner in this pearch than in a middling cod-fish, and he ob-erv'd the small vessels, that compose the greatest part of the ulk of the fibre, lying as close together, as ever he observ'd hem in any kind of fish or flesh.

L M N O Fig. 10 represents a small portion of the muscular fibres of the fish, cut thro' transversely, after they were grown dry, and in their shrinking had been torn off from the small vessels that incompass'd them; the openings of the small vessels in these fibres were distinctly to be seen, but they appeared in such great numbers and were so exceedingly small, that it was impossible to represent them any otherways than by points.

In this figure are represented what we call the membranes, but which indeed are nothing else than a congeries of small vessels, which not only surround the fibres, but enter into their very substance. These in the shrinking and drying of the object upon the glass-plate had been torn off from the fibres, as may be seen at PPP.

When this was done, M. *Leewenhoeck* put a small drop of water, about the size of a pin's head, on this small portion of fibres, into which it immediately insinuated and swell'd them to the same bigness, as when they were first laid upon the plate: After which the designer drew them, as they then appeared to him, omitting to represent the small vessels and only designing the circumference of every fibre, as appears at Q R S T, Fig. 11.

M. *Leewenhoeck* then split a grain of millet thro' the middle, and placing one half of it upon the glass, hard by the portion of fibres, represented in Fig. 10. the designer observ'd, that the half of the grain of millet-seed as represented by Fig. 12. appeared larger than the portion of fibres; as did likewise another person who view'd them: By which one may easily imagine, in how small a space that number of fibres is comprehended, each of which consists of so many vessels.

He likewise made his observations on the muscular fibres of a pike, roach, scar and flounder; in each of which he found the fibres compos'd of small vessels, like those of a cod and pearch.

He took the largest dried sprat he could procure, being a little more than five inches long, and he found that the fibres of a sprat were but little thinner than those of the large pearch above-mentioned; and that the vessels of which the fibres were compos'd, were nearly as numerous, as in the fibres of the pearch.

From these observations some may be apt to conclude, that the muscular fibres of land-animals are of the same thickness with those of fish: But for the satisfaction of such as have not seen

een the objects here ſpoken of, he caus'd a ſmall portion of he muſcular fibres of a large ox to be delineated, as they ppear'd thro' the ſame microſcope with the former, to ſhew he thickneſs of the dried fibres, and the veſſels which comoſe them, as is repreſented by XYZ Fig. 13.

The deſigner obſerv'd in the tranſverſe ſection of one of heſe fibres, 25 veſſels.

Upon cutting tranſverſely ſome of the muſcular fibres of a mall ſmelt, of the length of about two joints of his finger, e placed them before a microſcope, and obſerv'd that theſe ibres were not only twice as thick as thoſe of an ox, but likeviſe provided with as great a number of veſſels, as the fibres f other fiſh.

Upon thus obſerving that the muſcular fibres of fiſh were nuch larger than thoſe of land-animals, he caſt about, for the eaſons of this diſproportion; but all the ſatisfaction he could neet with was, that as fiſh ſwim in the water, their muſcular ibres need to exert very little force, in order to ſupport their odies therein, becauſe they are very nearly of the ſame ſpeific gravity with the element in which they ſwim; all the orce they exert is in their progreſſive motion, in purſuit of heir food: Whereas the muſcular fibres of land-animals xert a conſiderable force, not only in ſupporting and moving heir bodies, but in carrying burdens and in other labour they re put to: And we muſt allow, that the ſmaller and finer he fibres are, to make a body of any determinate thickneſs, he ſtronger will be the compoſition; and therefore, the muſles in fleſh muſt be ſtronger than thoſe of fiſh.

The wonderful and amazing ſtructure of theſe minute parts n the muſcles of beaſts and fiſh, which never enter'd into our houghts, juſtly claims our admiration; and doubtleſs, there re ſeveral other ſurpriſing things in them, which will, perhaps, for ever eſcape our penetration.

M. *Leewenhoeck*, taking out a little of the mealy ſubſtance f a boil'd grey pea, laid it before a microſcope, where it ppear'd to conſiſt of ſuch particles as are found in rats dung; very one of which parts conſiſted of a great number of very mall particles: But he could not diſcover any membranes enelopping thoſe parts; whence he concluded, all theſe membranes were deſtroy'd and diſſolv'd by the hot water.

Upon this, he took another grey pea, which had not been oil'd, and he cut it into very thin ſlices; when he not only bſerv'd the membranes, in which the parts of the mealy ſub-

ſubſtance had been incloſ'd; but likewiſe found, that thoſe membranes conſiſted of nothing elſe but a great number of very ſmall veſſels like the membranes (as they are commonly call'd) which ſurround the muſcles and muſcular fibres in beaſts and fiſh.

Obſervations on the Seeds of Plants; by the Same. Phil. Tranſ. N° 368. p. 200.

M. *Leewenhoeck* having often turn'd his thoughts to obſerve the membranes in which the ſubſtance of meal or flower is incloſ'd, like little packets in cells or boxes, which is alſo the caſe of all kinds of beans, peaſe, wheat, barley and other grain; he at length with aſtoniſhment diſcover'd very plainly, that what he calls the membranes were endu'd with an unſpeakable number of little holes, thro' which, in ſeveral places, one might perceive the light; which holes, we muſt ſuppoſe to be nothing elſe but ſmall veſſels, torn or cut off; and which partly compoſe the membranes he calls little cells, and partly ſerve for the production of the *farina*, of which there are vaſt numbers of particles in a pea or bean; each of which, as ſmall as they are, he ſuppoſes, receives its increaſe from a ſmall veſſel, which proceeds from the aforeſaid cell, and is imperceptible by reaſon of its ſmallneſs.

Theſe veſſels, of which the little cells or caſes do moſtly conſiſt, are more eaſy to be diſcovered in beans and peaſe, than in any other ſort of pulſe or grain, but in wheat the veſſels are with difficulty traced in the cells; and M. *Leewenhoeck* has been oblig'd to make a great many obſervations and experiments before he could fully ſatisfy himſelf, that he ſaw the torn or broken veſſels; the reaſon of which is, that the ſmall veſſels of which the cells or ſkins of the grains of wheat are compos'd, are exceeding thin and brittle.

Moreover, he found, upon obſerving the veſſels, of which the cells are compos'd, that ſeveral of the globules in wheat were broken in pieces in the operation; and that in one of thoſe ſingle globules there were other ſmall globules incloſ'd.

He likewiſe obſerv'd, that the membranes or little cells in barley, in which the globules or parcels of the meal are incloſ'd and receiv'd their nouriſhment, are thicker and ſtronger than thoſe of the wheat.

Tho' M. *Leewenhoeck* concludes, that almoſt all ſeeds and grain, as well as their membranes or ſkins, are of one and the ſame

me texture and configuration; yet for experiment ſake, he ok a large almond, and cutting off ſeveral thin ſlices thereom, he took out, as much as poſſible, the ſubſtance that lay the little cells; and viewing them as nicely as poſſible with microſcope, he obſerv'd, that thoſe cells, in which the oil the ſaid almond was for the moſt part contain'd, did alſo nſiſt of nothing but ſmall veſſels.

He intended, had it been practicable, to view the ſmaller rts of ſeeds, in order to find out, whether the little cells, in hich the farinaceous matter lay, were likewiſe compos'd of all veſſels; but he changed his mind, imagining that his tempts therein would be ineffectual, by reaſon of the ſmallſs of thoſe little cells; and conſequently of the veſſels, of hich thoſe cells are compos'd; tho' he does not queſtion, but hat we diſcover in the larger ſeeds is analogous to the ſtrucre of the ſmall ones.

Now when he perceived that the above-mentioned little lls proceeded from the bark or ſkin, which ſurrounds the rnel of the ſeed or grain, he was thinking, that as the ealy ſubſtance receives its increaſe from the veſſels, which e in the little cells; and as the plant is form'd between the lls, during the time that the ſeed lies in the earth; and as e little orifices in the ſkin of animals and fruit, are form'd order to diſcharge thereby the ſuperfluity of their moiſture, d ſhut in ſuch a manner, that no moiſture or common air n get into the ſame, as he formerly affirm'd; ſo on the conary, the orifices of ſeeds are form'd in ſuch a manner, that veral of their ſmall veſſels admit moiſture to paſs inwards, d accordingly water is driven into them by the preſſure of e air, which cauſes the ſeed to ſwell: Upon which a warmth d fermentation ſucceeding in the ſeed, it requires a larger ace; and by the particular formation of the particles, hich lie in the cells, and have deriv'd their increaſe thereom, the mealy ſubſtance of which they conſiſt, is partly drien out of them into the body of the young plant; which by is means increaſes ſo much in bulk, that the root can now pply it with nouriſhment from the earth; at which time the ed is found to be diminiſh'd in its bulk.

An

An Account of ſome Experiments relating to Magnetiſm, *by Dr.* Brook Taylor. Phil. Tranſ. N° 368. p. 204.

DR *Taylor*, after having given an account of an experiment made with a large magnet in the Repoſitory of the Royal Society (which experiment is deſcrib'd in *Phil. Tranſ.* N° 344) goes on with the ſame ſubject, as follows.

If it were known what point within the ſtone, and what point in the needle are the centers of the magnetical force, it would be eaſy to find the true forces of the magnet at all the diſtances obſerv'd: For want of that knowledge, the Dr. has computed the forces from the center of the needle and the extremity of the loadſtone; and finds, that at the diſtance of nine foot, the force alters faſter than as the cubes of the diſtances, whereas at the diſtances of one and two foot, the force alters nearly as the ſquares. To try whether the law, by which the magnetiſm alters, could be reduced at all diſtances, to any one certain power of thoſe diſtances, he ſought thoſe points in the needle and ſtone, which being us'd as the centers of the power, might have that property. But in that caſe he found the center of the ſtone muſt be carried quite out of its figure, to make the diſtances large enough for this purpoſe: Whence it ſeems to be evident, that the power of magnetiſm does not alter according to any particular power of the diſtances; but decreaſes much faſter in the greater diſtances than it does in the near ones.

This ſeems to be confirm'd by other experiments the Dr. made; the firſt experiment was this, he made a needle $\frac{3}{4}$ of an inch long, of very fine ſteel wire (a foot length weigh'd but a grain) which he lengthened by ſticking a light piece of ruſh to it; ſo that he could obſerve the direction of the needle in all the trials with a radius of two inches: Inſtead of a magnet he made uſe of a touch'd needle of ſteel wire, which he ſet on a perpendicular to the horizontal plane he made the obſervations on, by means of a frame, to tranſport it from one place to another; the north end of the needle being placed downwards and made a little ſharp, that it might mark the paper it was ſet upon in every poſition, by preſſing the top of the needle gently with the finger. The obſervations were made in this manner; after having taken notice of the natural direction of the ſmall compaſs needle, he brought the perpendicular needle as near it as he could conveniently, ſetting it in ſuch a manner, that a line from the upright needle

o the center of the compass might be perpendicular to the compass-needle. Then observing the same caution (which was convenient to make the center of the compass serve sufficiently well to be esteemed its center of power) he placed the upright needle at several large distances, every time marking the place in the manner already described; and observing the variation of the compass: By this means he got a curve pretty regularly and fairly drawn by points on the paper; and by examining this curve, compared with the variations of the compass, corresponding to its respective points, he found, that the magnetical power decreased faster at the greater than at the nearer distances: The Dr. observes, that at about two inches and $\frac{1}{4}$ distance, the force did not alter so fast as the squares; and at 10 inches distance (where the variation was one degree only) it altered faster than the cubes, the index of the power being about 3 and $\frac{1}{4}$. The needle of the compass was so short, that to suppose its center of force to be either in the middle or extremity thereof, would not alter the index of the powers of the distances, $\frac{1}{20}$ of an unite.

He made another experiment to the same purpose, with a compass needle made of a slight piece of straw, with a little piece of steel-wire, fastened to one end of it, which was always kept in the same position, being balanced between two perpendicular needles, one of which was moveable and the other fixed; the event was much the same as in the former experiment.

Endeavouring to find the true poles or centers of the magnetical force in touch'd needles, he made a needle of two inches long, of the fine steel-wire, which he touched with the south point of a small capt loadstone, applying the point of the cap to the extremity of the needle, without drawing it along: The needle so touched, being laid gently on the surface of a stagnant water, floated; he then applied to it successively the two ends of a touched needle, as near as he could, without letting the needles touch: The result was, that the floating needle rested under the respective poles of the other needle, marked with the small letters *n s*, as represented in Fig. 14. Plate IV. so that by one touch with the loadstone, which gave the needle a north pole at N. where it was touched, it acquired three other poles *s n s*, which may not, therefore, be improperly called its consequential poles: Having discovered these consequential poles, he made some other experiments to discover more of the nature of them, as represented in the Fig. The needles were all of them two inches long, made of the same fine steel wire; and the letter N or *n* and S or *s* denote north or south, belonging to the points marked; the

great letters denoting the points the loadſtone was applied to, and the ſmall letters ſhewing the conſequential poles.

As to the Dr's experiment relating to the hyperbola made by the ſurface of water between two glaſs-planes; vide *Phil. Tranſ.* N° 336.

Dr. *Taylor* took ſeveral very thin pieces of fir-board, having hung them ſucceſſively in a convenient manner to a nice pair of ſcales, he tried what weight was neceſſary (over and above their own, after they had been well ſoaked in water) to ſeparate them at once from the ſurface of ſtagnating water: He found 50 grains to ſeparate a ſurface of an inch ſquare; and the weight in every trial being exactly proportional to the ſurface, he was apt to think the experiment well made: The diſtance of the under ſurface of the board from the ſurface of the ſtagnating water, at the time they ſeparated, he found to be $\frac{1}{100}$ of an inch; tho' he thinks it would be found greater, if it could be meaſured at a greater diſtance from the edge of the board, than he could do it, the water riſing a little before it came quite under the edge of the board.

A Method of determining the Places of the Planets, *by obſerving their near appulſes to the fixed* Stars; *by Dr.* Halley. Phil. Tranſ. N° 369. p. 209.

OF all the celeſtial obſervations hitherto made, none are ſo capable of perfect exactneſs, as the near appulſes of the moon and planets to the fixed ſtars; ſuch as lately we had of *Jupiter* to two ſmall ſtars in *Gemini* and of *Mars* to the forehead of *Scorpio*: For, tho' the places of the ſtars have not hitherto attained an ultimate preciſion; yet theſe ſorts of obſervations are ever good and the places of the planets are thereby aſcertained, in proportion to the correctneſs of the catalogues, that may hereafter be made: But the ordinary number of the ſtars, with which the planets may be thus compared, being ſmall, the opportunities of obſerving are conſequently rare: Whence appears the great uſe of a compleat catalogue of teleſcopic ſtars, at leaſt within the limits of the zodiac; *viz.* that thereby theſe opportunities may be more frequent: And wherever ſuch obſervations have formerly been made to theſe ſmall ſtars, we may be enabled to find them out; and by determining their places, to be alſo certain of the places of the planets; of which the Dr. gave a notable inſtance in finding the place of the great comet in 1680, in its firſt appearance, even before it had a tail viſible to the naked eye, of which

which an account is given in *Phil. Tranſ.* N° 342. And ſince the Royal Obſervatory at *Greenwich* has been put under the Dr's care, he has endeavoured to put himſelf into a condition to ſupply the many and great vacancies to be met with in the preſent zodiac; and particularly, he has ſought out and ſettled the places of two teleſcopic ſtars, to one of which *Jupiter* was obſerved by *Galilæo* to apply at the beginning of *March* 1610 N. S. and which is the very firſt obſervation of that kind that was made with the teleſcope, vide *Nuncius Syder.* p. 27, *edit. prin.* 1610. On the 28. of *Feb.* an hour after ſun-ſet, a ſmall fixed ſtar was in conjunction with the fourth ſatellite (as it ſince appears to have been) being then eaſtwards of the planet. The next day *March* 1ſt at the ſame hour, the center of *Jupiter* was in the angle of an equilateral triangle with the fourth ſatellite and the ſtar: And again *March* 2. *Jupiter* being retrograde, had paſt the conjunction of the ſtar; and a line from the ſtar, perpendicular to that of the ſatellites, fell on the firſt ſatellite then two minutes to the weſt of the planet, and in Lat. the ſtar was more ſoutherly than the ſatellite 8 mintues: The Dr. found out this ſtar, by the direction of the place of *Jupiter* at that time; and by comparing it with others in the catalogue, having nearly the ſame declination, he ſettled its place in 13° 34′ and ½ of *Gemini* to the time of the *Britiſh* catalogue with 5′ S. Lat.

Another remarkable obſervation of *Saturn* is recorded in *Riccioli Aſtron. Reform.* p. 286, ſaid to have been made at *Modena* by the Marquis *Malvazzo* on *July* 3, 1662, N. S. when the eaſtern *anſa* of *Saturn* touched a fixed ſtar: By the then place of *Saturn*, the Dr. looked out for this ſtar to which *Saturn* was then very near; and after the ſame method he ſettled its place in the beginning of the year 1690 (the *epocha* of the *Catal. Brit.*) in 29° 34′ of *Scorpio* with 2° and ½ a minute N. Lat. By this it will appear, how defective the obſerved place of *Saturn* is ſtated in *Riccioli*, there being an error of upwards of 7′ in the Lat. thereof.

An Obſervation of a Parhelium; *by the Same.* Phil. Tranſ. N° 369. p. 211.

OCTOBER 26. 1721, Dr. *Halley* being on the river, coming up to *London* about ½ an hour after 10 o'clock in the morning; the ſun being then about 20 degrees high, he obſerved a circle about the ſun, which is by no means unuſual, when the air in chilly weather, ſuch as it was then, is replete

with snowy particles; which circle was of the size it always appears in, about 23 degrees from the sun and faintly tinged with the colours of the *iris.* When this circle happens, the Dr. always looks out to observe, whether any other of the phænomena, that sometimes attend it, do at that time appear; such as parhelia and other coloured circles, concentric with the sun; and sometimes, as he once observed it, excentric; as also a white circle round the zenith, of equal altitude with the sun: But at this time the air being thickened with a hazy vapour and the smoke of *London*, he could only observe to the eastward a luminous white patch, which, for about 20 minutes shone very conspicuously thro' the thick air, about two degrees in diameter, as near as he could estimate, and about the same altitude with the sun; and from it towards the sun, there seemed to issue a long white tail much narrower than the mock-sun; but which he took to be a segment of the white circle, which he once observed entire at *London*: Had the air been clear, the Dr. doubts not but much more of the phænomena of the parhelia might have been observed at that time: But how to explain these appearances and account for the magnitude of these circles is what seems still to be wanting.

An Account of two Mock-suns *and an arch of a* Rain-bow *inverted; together with an* Halo *and its brightest arch, observed at* Lyndon *in the County of* Rutland; *by Mr.* Whiston. Phil. Trans. N° 369. p. 212.

OCTOBER 22. 1721, about 10 o'clock in the morning, Mr. *Whiston*, being at *Lyndon* in the county of *Rutland*, after an *aurora borealis* the night before (wind W. S. W.) he observed an attempt towards two mock-suns, as he had done at some other times: About ½ or ¾ of an hour after, he went to view the heavens and then found the appearance compleat, when two plain parhelia or mock-suns were to be seen tolerably bright and distinct; and that in the usual places, *viz* in the two intersections of a strong and large portion of an halo (as represented in Fig. 15. Plate IV.) with an imaginary circle, parallel to the horizon, passing thro' the true sun: This circle he calls imaginary, because it was not itself visible, as it sometimes has been at such appearances: Each parhelion had a tail of a white colour, and in direct opposition to the true sun; that towards the east was 20 or 25 degrees long; that towards the west about 10 or 12 degrees; but both were narrowest at the remote extremities: The mock-suns were evidently red

towards

owards the ſun; but pale or whitiſh at the oppoſite ſides, as was alſo the halo: Upon caſting his eyes upward, he obſerved n arch of a curious inverted rain-bow, about the middle of he diſtance between the top of the halo and the vertex, that s, when allowance is made for the uſual unequality, that appears between the ſame number of degrees, nearer to and remoter from the vertex: This arch was as diſtinct in its colours, as the common rain-bow; and with the like allowance s before, of the ſame breadth: The red colour was on the onvex, and the blue on the concave of the arch, which eemed to be about 90 degrees long; its center in or near the ertex: On the top of the halo was a kind of inverted bright rch, tho' its bend was not diſtinct: The lower part of the alo was among the vapours of the horizon and inviſible. The ngles, as more exactly meaſured next day, near noon (when he ſame appearance returned again, but more faintly) were s follows; the ſun's altitude was 22° and $\frac{1}{3}$; the perpendicular ſemidiameter of the halo 23° and $\frac{1}{3}$; the diſtance of the ainbow from the top of the halo 25° and $\frac{1}{3}$; the ſemidiameter of the arch of the rainbow, ſuppoſing the vertex its center, 1°. The phænomenon laſted each day for 1 hour and $\frac{1}{2}$ or two hours. What was moſt remarkable on the 23. was that he wind, which on the 22. was almoſt inſenſible, was now become ſenſible and changed to N. N. E. ſo that the halo was enſibly become oval; its ſhorter axis parallel to the horizon; and the two mock-ſuns, which were then but juſt viſible, eſpecially that on the eaſt, were not in the halo, but a degree or two without it, which Mr. *Whiſton* aſcribes to the unuſual ſhortneſs of the horizontal diameter; and this poſition of the mock-ſuns does not appear to have been taken notice of by any, tho' it was now very ſenſible.

October 26. About 9 o'clock in the morning, as Mr. *Whiſton* was coming in the *Northampton* coach towards *London*, the halo returned larger and clearer than before; and the two mock-ſuns juſt attempted an appearance therein, as on the 22. but the air becoming thicker and thicker towards rain, he ſaw them no more.

Auguſt 30. before, he obſerved at *Lyndon* in *Rutlandſhire* a remarkable halo, whoſe upper part had its inverted arch reddiſh within and pale without, but brighter and more vivid than ever he obſerved it in his life; and at the ſame place *Sept.* 11. in the evening there was the brighteſt and moſt remarkable *aurora borealis*, with its unaccountable motions and ſhiftings,

ſhiftings, that ever he obſerved; excepting that original one March 6. 1715-16; this *aurora* was ſeen in *Northamptonſhire*, at the *Bath* and elſewhere; the vertex of the columns, which ſhot upwards, was not his vertex, but evidently 15 or 20 degrees diſtant towards the ſouth; and in *Rutland* the wind was north, at the *Bath* weſt, and in *Northamptonſhire* ſouth, all at the ſame time.

As to thoſe northern lights, Dr. *Halley* ſoon after their firſt appearance, communicated his thoughts to the public in a former *Tranſaction*, and Mr. *Whiſton* in a ſmall pamphlet, publiſhed about the ſame time; and as to the halo's, mock-ſuns, inverted arches of rain-bows and other phænomena of the like nature; M. *Huygens* has very accurately explained them in his *poſthumous works* from p. 293. to p. 366. and Sir *Iſaac Newton* has touched upon them in his *Optics* 1. *edit.* p. 134. As to the unuſual frequency of the northern lights of late Mr. *Whiſton* was far from ſatisfying himſelf.

Obſervations on the Generation of Plants; *by Dr.* Blair. Phil. Tranſ. N° 369. p. 216.

WHAT Dr. *Blair* had advanced in his *Botanic eſſays* is fully confirmed by experiments made by ſome curious gardeners, particularly, Mr. *Philip Miller*, who writ him an account 1. That *Nov.* 11. 1721, in purſuance of the Dr's advice, he ſeparated the male plants of the ſpinage from the female; the conſequence was, that the ſeeds ſwelled to the uſual bigneſs; but that they did not grow when he ſow'd them: He examined the ſeed, and found they wanted the *punctum vitæ*, which perhaps might have been the caſe with M. *Geoffroy*; but if not, the female embryo's might have been impregnated another way, as he tried with 12 tulips, which he ſet by themſelves about ſix or ſeven yards from any other; and as ſoon as they blew, he took out the *ſtamina* ſo very carefully, that he ſcattered none of the duſt; and about two days after, he obſerved bees working on tulips in a bed, where he did not take out the *ſtamina*; and when they came out, their bodies and legs were loaded with the duſt; he obſerved the bees fly into the tulips, where he had taken out the *ſtamina*; and when they came out, he found they had left enough behind them to impregnate theſe flowers; for, they bore good ripe ſeed; by which he is perſuaded, that the *farina* may be carried

ied from place to place by inſects; and when they happen ipon a flower, whoſe *uterus* is capable of being impregnated by ſuch a duſt, it may be thus effected.

The Dr. is of opinion, that this will not ſuit with Mr. *Morland*'s ſcheme: For, tho' we may ſuppoſe the *ſtamina* of every flower to be loaded with a due proportion of the *farina*; yet his accidental conveyance of it to a neighbouring flower may be rather leſs than greater than is neceſſary: So that if wanting then thoſe embryo's, which had not received the determined particle into their boſom, muſt be defective in bulk or barren in growing; but here all were equally filled.

2. Mr. *Miller* in another letter, dated *October* 19. 1721, informs the Dr. that he bought a parcel of *Savoy* ſeeds, which he ſowed, and planted out the plants; but was ſurpriſed at the production. For, he had half of them red cabbages; ſome ſavoys with red ribs; and ſome neither one ſort nor the other, but a mixture of all ſorts together in one plant. The ſame thing happened to the gardener of whom he had the ſeed, but he did not know how; for, he was ſure he took ſpecial care in ſaving of the ſeed; he ſowed them under a ſouth-weſt hedge, and planted them in the following manner; firſt a dozen of white cabbages, then a dozen of ſavoys and then a dozen of red: Upon which Mr. *Miller* immediately thought, that this was owing to the effluvia impregnating the *uterus* of each other; and it is very common for our gardeners to plant white and red cabbages together for ſeed; and they are as often diſappointed by having a degeneracy of both kinds, which they attribute to the ſoil; and they ſend to *Holland* for a freſh ſupply of ſeed and affirm that our ſoil will not continue that ſort good.

This experiment is a moſt convincing argument for the effluvia; for, did each grain of the *farina* enter the *piſtillum* to its proper *uterus*, this mongrel kind would never be produced: For, if the individual plant be in each grain of the male *farina*, how can it be ſo far diſmembered, as that one part ſhall go to the making up the ribs of red cabbage and another to compoſe the reſt of a ſavoy plant.

Analogous to this is what the Dr. obſerved of a ſpaniel bitch, of a good kind; when ſhe became proud, care was taken to let her have good dogs; the litter ſhe produced conſiſted of puppies, ſome pieball'd, like one of the dogs that had lin'd with her, of the ſame ſhape, colour and ſpots; others

others like another; and a third partaking of both, with spo[ts]
of the bitch interspers'd.

This is a farther confirmation of what the Dr. has advanc[ed]
Essay 4. where p. 310. he asserts, that several *fœtus's* par-
take equally of male and female: But here two males concu[r]
with one female in the composition of a fourth body, ma[de]
up of all the three; and one seed produces a cabbage, con-
sisting of three different species, which could never happe[n]
did these organiz'd *animalcula* or small grains of the *farin[a]*
become a *fœtus* or contain the *folia seminalia* of a plant:
This is sufficient to answer what the ingenious Mr. *Bradley*
has so strenuously contended for, in his *Works of Nature* p. 9.
seqq.

It is worth while to consider, how far this observation may
lead us into the infinite variegations and stripes, not only in
annual flowers, such as poppies, *consolida regalis* and bottles,
but also in perennial roots; such as *auricula's*, cowslips, &c.
of a lower size, which is hinted by Mr. *Bradley*, having
receiv'd that notion from the ingenious M. *Du Bois*; and in
plants of a larger size, not of a bulbous but carnous root,
such as columbines; where there is a vast variety: And in
this plant it is most especially to be observ'd, that tho' the
native one, from which all the other seem only to be varia-
tions and not determinate species, be of a blue colour, consist-
ing of 10 alternate *petala*, viz. five corniculate and five plain:
Yet into how many other kinds of flowers is it subdivided?
Such as pale yellow, with bluish red, purple, dark double
stripes, blue, blackish red, *&c.* Some with corniculate *petala*,
and some only with plain; and how in single flowers it imitates
all the colours observable in a pidgeon.

It is worthy consideration, whether the *farina* may do this;
since it does not appear, that there has been much art us'd
in making these flowers break, as tulips, or to cultivate a set
of breeders; but that a richer soil may produce a double
flower; and a suitable loam may produce the variety of
colours: The *farina* from several flowers may occasion the
stripes; and the *stamina* arising from the plain *petala*, rather
than the *cornicula*, pouring out the *farina*, may cause the
flowers with the plain *petala*. So that were this to be extended
to a great many other plants and were there proper observa-
tions made upon them, considerable improvements might be
made in this doctrine of the sexes of plants: After the flowers,

we-

e might consider next the variegation of the seed of some lants, particularly the *phaseoli*, whose various spots and olours, and even their bigness too, may very much depend pon the effluvia from the *farina*, when several kinds are own together: For, if we consider three plain colours, as a hite, red and dark blue, we may observe how many descen-ants, and what a variety of spots may proceed therefrom The upines may likewise in some measure be brought in here; nd perhaps, the *Medica cochleata falcata lunata* may be ultiplied in its variegations after the same manner.

Mr. *Miller*, being persuaded to it by an ingenious gardiner, ull'd off all the male flowers of some melon plants, as soon s they appeared; but instead of finding, as his friend nform'd him, that these flowers exhausted the nourishment om the fruit, he found, that without these flowers none of e melons would grow; so that he was depriv'd of the fruit e expected.

As this experiment is a plain indication of the necessity of e *farina*; so it confirms the use the Dr. has assigned to the eaves, *viz.* that the nutritive particles by entering their capil-ries and returning again, may be more attenuated: So here e *petala* of the male flowers may serve for the same pur-ose; for, by the largeness of the *tubuli* in these *pomiferæ andentes*, a gross viscid sap is receiv'd, which even the leaves emselves are not sufficient to attenuate, so as to be fit for mposing the more subtile part of the fruit; till by repeated rculation thro' the *petala* of the male flowers it may be ender'd fit for such a purpose: It is true, that the female owers upon the top of the *rudimenta fructûs* may in some easure serve for this purpose; but as the male flowers are, enerally speaking, more numerous than the female; so their eing remov'd must deprive the embryo's of a very great ssistance towards being perfected: The Dr. farther adds, at the orifices of the pedicles, when the flowers are pull'd ff, must lose so much of the sap, that the whole plant must e thereby impoverish'd in such a manner, as not to be able bring forth the design'd fruit; and all this, besides the want f the considerable supply of the *farina fœcundans*.

An Observation of an extraordinary height of the Barometer; *by Mr.* George Graham. Phil. Transf. N° 369, p. 222.

ON the 21. of *December* 1721, Mr. *Graham* observing the barometer much higher than usual; between 7 and 8 o'clock that evening he fill'd a tube with very clean quicksilver, and found the height to exceed somewhat 30, 7 ½ inches: By 8 o'clock next morning, a wheel-barometer, which hung in the same room, had risen one tenth of an inch higher than it was the night before, when the experiment was made; at 10 o'clock, one fifth of an inch more: At which time it was at the highest, being a little above 30, 8 ½ inches; about 12 at noon it was sensibly lower and continued falling all the rest of the day.

When the lower end of the tube was first immers'd in the cistern, the quicksilver adher'd for some time to the crown of the glass; but upon shaking, it fell to the above-mentioned height.

A Caution to be us'd in examining the specifick Gravity of Solids, when weigh'd in Water; by Dr. Jurin. Phil. Transf. N° 369. p. 223.

THAT the experiments for finding the specific gravity of solid bodies may be made with exactness so as to draw any inferences from them in natural philosophy, Dr. *Jurin* gives the following caution, *viz.* that when a dry porous body is to be weigh'd in water, in order to discover its specifick gravity; it is necessary, by some means or other, to extricate the air out of all the small pores and vacuities within it, that the water may have free liberty to pervade them; unless this care be taken, it must needs happen, that the air, which possesses those small cavities and keeps the water out, will render the solid of less weight in the water; and consequently of less apparent specifick gravity than it really is.

The best way of avoiding this inconveniency is, to set the vessel of water, in which the solid body is immers'd, under the receiver of an air-pump, and to exhaust the air out of the body by that means; which will be more easy and exactly done, if the water be first heated over the fire: And where the conveniency of an air-pump cannot be had, the same thing may be done almost as well, by letting the solid body continue some time in boiling water over the fire.

Ex-

But no ſolid body muſt ever be put into hot water, that will n any meaſure diſſolve or give a tincture to the water.

One inſtance of the neglect of this caution may be obſerv'd in he accounts we have of the ſpecifick gravity of the ſtones ex- racted from human bladders, which have been commonly found be but about half, and ſome of them no more than $\frac{1}{4}$ part eavier than an equal bulk of water. From this it has been too aſtily concluded, that theſe ſtones are very improperly call'd by at name, as not at all approaching to the ſpecifick gravity of ven the lighteſt real ſtones, we have any account of.

Whereas it is much more reaſonable to ſuppoſe, that thoſe ones, which have been found to be ſo light, were ſuch as had een for a conſiderable time extracted out of the bladder; and nſequently, had loſt much of their weight by the evaporation f the urine, with which they had at firſt been ſaturated, and at they had afterwards been tried without the caution above- entioned. The Dr. would, therefore, recommend it to thoſe, ho ſhall examine the ſpecific gravity of the human *calculus*, at they would either try the experiment upon ſtones freſh ex- acted out of the bladder, or elſe uſe the aboveſaid method, to xtricate the air out of their cavities; if they do this, the Dr. ſure they will meet with ſome *calculi* (as he himſelf has done) ceeding the weight of ſome ſorts of burnt earthen ware and abaſter; and approaching very near to that of brick and the fter ſort of paving ſtone: But it is not to be expected, that ey ſhould entirely equal the ſpecific gravity of ſtone, found in e earth; becauſe the mixture of ſome portion of the animal oil nd volatile ſalt, with the ſtony ſubſtance of the human *calculi*, uſt needs leſſen the ſpecific gravity of the whole concrete.

Dr. *Jurin* was very much ſurpris'd at the following obſerva- ion, he accidentally diſcovered, to wit, that the ſubſtance of all ood (as oak, fir, *&c.*) is ſpecifically heavier than water: To revent being miſunderſtood, the Dr. obſerves, that in wood and ther vegetables, there are two ſorts of veſſels; one of which onvey the ſap; and the other contain only air, for which reaſon ey are call'd air-veſſels: When wood floats or ſwims in water, is effect is not owing to the lightneſs of the ſubſtance of the ood, but only to its being buoy'd up by the air contain'd in the id veſſels: For, when the air is exhauſted out of thoſe veſſels, nd inſtead thereof the water has inſinuated itſelf into them, the ood will ſink to the bottom; as is evident in ſmall chips or avings of wood, by means of the air-pump or an infuſion in oiling or even in cold water for a ſufficient time; and the ſame

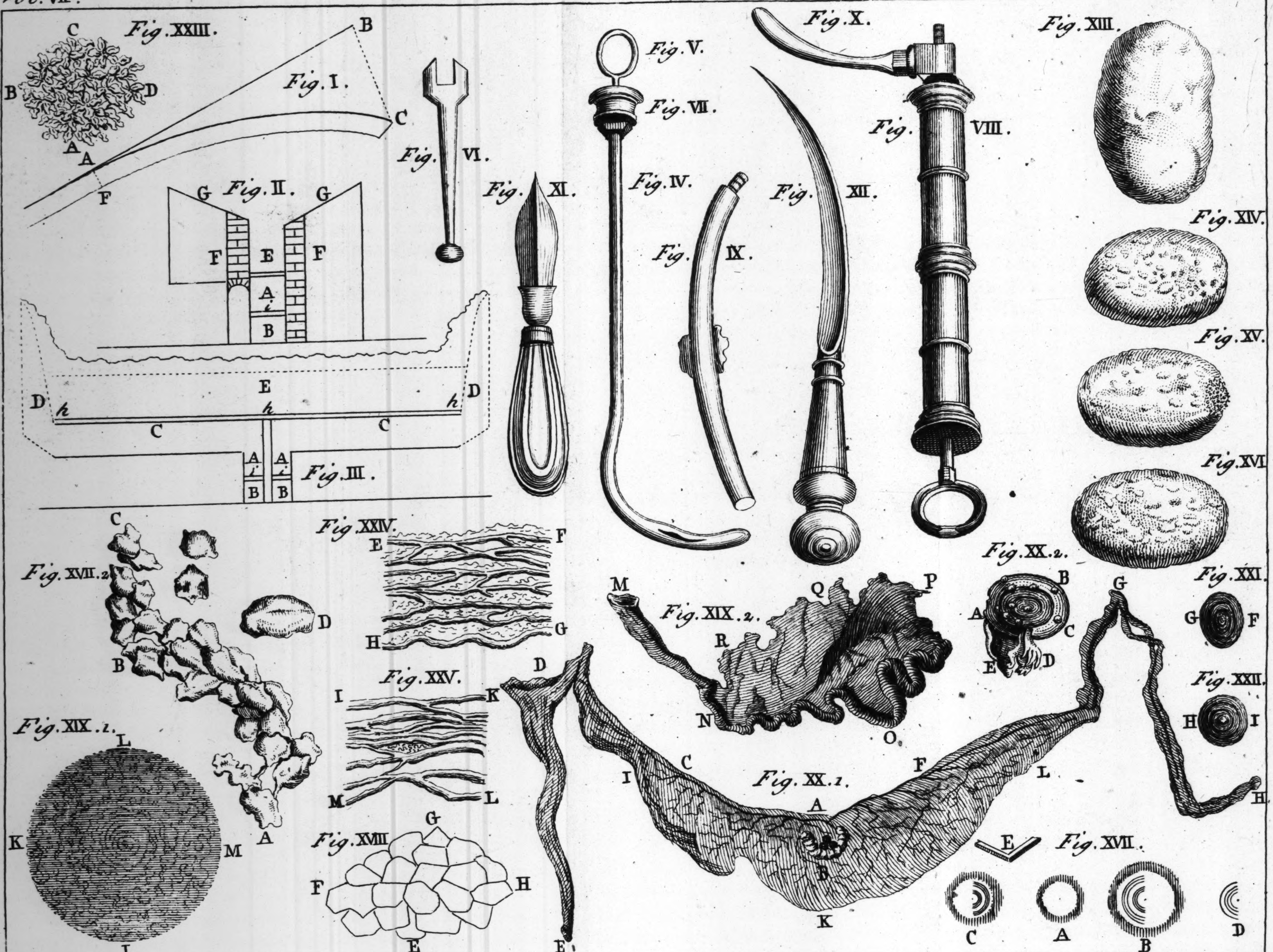
is found to be the case in the roots, stalks, leaves and seeds of as many other vegetables as the Dr. had hitherto tried, cork only excepted; in which last he had no reason to expect it, considering the particular structure of its substance, as described by the learned Dr. *Hook* in his *Micrographia*.

An Ossification of the Crural Artery; *by Mr.* Edward Naish. Phil. Transf. N° 369. p. 226.

MR. *Consett* of *Cleveland* in *Yorkshire*, 67 years of age, who had all his life before enjoyed a perfect good state of health, sent for Mr. *Naish* on account of a mortification which began about a month before upon one of his toes; and by gradual advances in that time had reached half way up his leg; and this without any manifest cause: Mr. *Naish* found him in this condition, *viz.* with a perfect mortification or *sphacelus* of his foot and half his leg: The patient saw himself dying daily by piece-meal, but heart-whole, as he expressed it, and with a pretty good pulse. Mr. *Naish* proposed amputation as the only remedy, which the patient readily consented to.

The leg being cut off at the usual place (which was four inches above the mortification) about two or three ounces of blood issued out from the muscular part; but upon slackening the turneket, in order to look for, and tie up the artery, not one drop of blood flowed out, to Mr. *Naish*'s great surprise; and upon feeling the extremity of the artery, he found it hard and callous; however, he secured it by a ligature, as usual, and dress'd the stump. The patient (who had borne the operation with the greatest resolution) being put to bed, Mr. *Naish* was desirous to examine the leg; and having dissected the artery, with its two considerable branches, as far as the *tarsus*, he found them for the most part ossified; that is to say, the trunk, where it was amputated, was ossified about $\frac{2}{3}$ of its circumference: About $\frac{1}{4}$ of an inch lower, the whole was bony, leaving so small an orifice, that it could only admit of a hog's bristle: A little lower, it was bony on one side and membranous on the other; and then again entirely bony; here and there, for the breadth of a barley-corn, there was no bone at all. Mr. *Naish* opened about two inches of the internal branch immediately above the *malleolus*, it appearing blacker than the rest; after it had been washed he found therein about two or three drops of coagulated blood, and now that it was expanded and dried, it was one entire *lamina* of bone, as thick as the shell of a pidgeon's egg and of an uneven surface: He dissected three ramifications of this internal branch into the foot only

PLATE. VI.



G. Smith del.

Hulett Sculp.

nly one of them had a very small bone in it, about ½ an inch from the trunk: The other large branch, running on the ligament, hat ties the fociles together, was not so much ossified as that lready described.

This ossification (the compleatest Mr. *Naish* ever heard of) was, doubtless, the cause of the mortification, and of the death f the patient, which followed four days after the amputation.

This bony *lamina* or shell was on the inside of the tunicles or oats of the artery. Mr. *Naish* doubts not but that these cases re more common than is imagined: For, when we see mortifications seize the extreme parts of aged people, we commonly attribute them to a decay of nature, or an extinction of the vital warmth, tho' he takes this to be oftener the cause; and he is the more apt to think so, from two or three parallel cases he had been acquainted with in his own practice.

An Account of a Rainbow *observed on the Ground; by Dr.* Langwith. Phil. Transf. N° *369*. p. *229*.

SEPT. 7. 1721, about 9 o'clock in the morning Dr. *Langwith* riding with some others over *Port-mead* near *Oxford*, nd the morning misty and grass very wet with the dew; they had not been long out, before the air cleared up and the sun began to shine very bright: Soon after, they observed a rainbow on the ground, whose colours were near as lively as those of the common *iris*, extending for some hundreds of yards; the colours were so strong, that it might have been observed much farther, had it not been terminated by the bank and hedge of the field: It continually changed its place as they moved along, which commonly happens in other rainbows; the more remarkable particulars were, as follows.

1. That its figure was not round, but oblong; being as Dr. *Langwith* took it, a portion of an hyperbola. 2. That its convex part was turned towards the eye, and the vertex at a small distance before them. 3. That the colours took up less space, and were much more lively in the nearer than remoter parts thereof.

These phænomena may be easily accounted for, by comparing this *iris* DCE Fig. 16. Plate IV. with the common *iris* *k i* E *e*, formed by drops falling in the air at a small distance from the eye of the spectator H, and touching the ground with the lower part of its arch in E, the vertical point of the *iris* DCE; produce the cone H *k i* E *e*; its intersection with the plane of the horizon will give the figure of the *iris* DCE. Hence it follows.

1. That

1. That as the angle eHG happens to be greater, equal to, or leſs than 90 degrees, the figure will be an hyperbola, parabola or ellipſis. 2. That as the ſun was about 30 degrees high, when they view'd the phænomena, the *iris* was an hyperbola. 3. That the arches of the ſame *iris*, conſiſting of colours of different refrangibility, may alſo in ſome caſes be different ſections of the cone. 4. That ſince the angle eHF is always given; from the height of the point of view HG and the ſun's altitude SLA, the dimenſions of theſe *iris*'s are eaſily determined.

An Account of the Pores of the Leaves of Box; *as alſo of the* Down *of* Peaches *and* Quinces; *by M.* Leewenhoeck. Phil. Tranſ. N° 369. p. 231. *Tranſlated from the* Latin.

M. *Leewenhoeck* pulled off a leaf from that ſpecies of box, called *palma ceres*, and tearing it to pieces he viewed it with his microſcope; after which, he could very diſtinctly obſerve theſe parts, thro' which perſpiration is performed; beſides a great many very ſmall pores, that tranſmitted the light; and which appeared to greater advantage, and more numerous, as the parts of the leaf were ſomewhat drier.

In the ſmall leaves of a box, that were partly green and partly withered, he perceived the pores ſtill more diſtinctly than he had ever done in any others; and the more diſtinctly to repreſent their great number, he laid a box leaf on a braſs ruler divided into ſeveral parts; and he found its length equal to $\frac{8}{10}$ parts of an inch (ſuppoſing an inch divided into 10 parts) and its breadth to $\frac{5}{10}$ parts.

Now ſuppoſing the figure of ſuch a leaf to be oval, and adding its length and breadth together, the ſum will be 13, whoſe half is 6 and $\frac{1}{2}$: Again ſuppoſing the ſame leaf, after adding its length and breadth together, to be round like a circle, its diameter is equal to 6 tenth parts and $\frac{1}{2}$ of an inch.

Afterwards M. *Leewenhoeck* laid a hog's briſtle cloſe by the ſaid leaf; and viewing it with his microſcope, he judged that 12 pores of the leaf, if contiguous to each other, would be equal to the diameter of the briſtle; and that 60 briſtles would make an inch; whence it follows, that each tenth part of an inch is equal in length to ſix diameters of a briſtle; but the ſemidiameter of a ſmall leaf of box to 19 diameters and a half of a briſtle; which 19 diameters and $\frac{1}{2}$ if multiplied by 12, the number of pores, the product will be 234, which, from what has been ſaid, is the length of a ſemicircle of box leaf.

In order to find the superficial contents of such a circle; we must the first place suppose with geometricians, that as 14 to 11, is the square of the diameter of any circle to the contents eteof: Hence it follows, that one superficies of a box leaf is rnished with 172090 pores; and since the other is furnished th no fewer, the whole number of pores for perspiration will 344180.

M. *Leewenhoeck* observed with his naked eye the down on aches, commonly called mountain-peaches; he placed before s microscope some pieces, he had cut off from the rind; and judged the down particles equal in number to the pores in e rind; and as he had formerly hinted, that the pores in fruits ened between two small lips, as it were, and were not quite und, but somewhat oblong; so in like manner the down, issu- g out of the said pores, is not quite round, but somewhat t; yet he observed a great many in the middle somewhat rrugated.

In order to shew the vast number of pores designed for perspi- tion, as also the exhaling humour, which emitted into the air, inspissated into oblong particles, but some longer than others, . *Leewenhoeck* caused delineate a small portion of a peach, as presented by A B C D E F G Fig. 17. Plate IV. where A B F G a little bit of the fruit and rind; B C D E F the down adher- g to the peach.

The bulk of the said little piece of peach may be estimated us; M. *Leewenhoeck* took three hairs of his face, and viewing em with his microscope; after dividing the length of the figure om G to A into 16 parts, the said length of the figure was und not to exceed eight diameters of a hair: Which if true, hat an incredible number of down particles must necessarily ver a peach?

Afterwards M. *Leewenhoeck*, viewing with his microscope me particles of a ripe quince, caused delineate the down there- , which is no less copious than on a peach, as represented by IKLMNO Fig. 18; where H I N O is a small portion of e pulp and rind; I K L M N the down exsudating therefrom, hich, tho' longer in quinces than in peaches, does not stand erect the former as in the latter, but is matted together.

Remarks

Remarks on ſome attempts towards a perpetual Motion; *by* *Dr.* Deſaguliers. Phil. Tranſ. N° 369. p. 234.

THE wheel at *Heſſe-Caſſel*, made by M. *Orfireus* and by him called a perpetual motion, has been ſo much talked of, on account of its ſurpriſing phænomena, that a great many have taken it to be actually a ſelf-moving engine, and accordingly attempted to imitate it as ſuch; Dr. *Deſaguliers* ſhews, that the principle, moſt of them go upon, is falſe and can by no means produce a perpetual motion.

They take it for granted, that if a weight, deſcending in a wheel, at a determinate diſtance from the center, does in its aſcent approach nearer to it; ſuch a weight in its deſcent will always preponderate and cauſe a weight equal thereto to riſe, provided it comes nearer the center in its riſe; and accordingly as itſelf riſes, will be overbalanced by another weight equal thereto; and, therefore, they endeavour by various contrivances to produce that effect, as if the conſequence thereof would be a perpetual motion.

The Dr. ſhews, that they miſtake one particular caſe of a general *Theorem*, or rather a *Corollary* of it for the *Theorem* itſelf, which is as follows.

Theor. If one weight in its deſcent, does, by means of any contrivance, cauſe another weight to aſcend with a leſs *momentum* or quantity of motion than itſelf, it will preponderate and raiſe the other weight.

Cor. 1. Therefore, if the weights be equal, the deſcending weight muſt have more velocity than the aſcending, becauſe the *momentum* is made up of the weight, multiplied into the quantity of matter.

Cor. 2. Therefore, if a leaver or balance, have equal weights faſtened or ſuſpended at its ends, and the *brachia* be ever ſo little unequal, that weight will preponderate, which is fartheſt from the center.

Scholium. This ſecond *Corol.* cauſes the miſtake; becauſe thoſe, who think the velocity of the weight is the line it deſcribes, expect that that weight ſhall be overpoiſed, which deſcribes the ſhorteſt line; and therefore contrive machines to cauſe the aſcending weight to deſcribe a ſhorter line than the deſcending weight does: As for example, in the circle A D B *a* Fig. 19. Plate IV. the weights A and B, being ſuppoſed equal, they imagine, that if (by any contrivance whatever) whilſt the weight A deſcribes the

he arch A*a*, the weight B is carried in any arch, as B*b*; ſo as o approach nearer the center in its riſing, than if it went up the rch BD; the ſaid weight ſhall be overpoiſed; and conſequently, y a number of ſuch weights, a perpetual motion will be produced.

This is attempted by ſeveral contrivances, which all depend n this falſe principle; the Dr. only mentions one, repreſented y Fig. 20. where a wheel with two parallel circumferences, has he ſpace between them divided into cells; which being carved, ill (when the wheel goes round) cauſe weights placed looſe in he ſaid cells, to deſcend on the ſide A A A, at the outer circumference of the wheel; and on the ſide D to aſcend in the line *bbb*, which approaches nearer the center and touches the inner ircumference of the wheel: In a machine of this kind, the eights, it is true, will move in ſuch a manner, If the wheel be rned round, but will never be the cauſe of the wheel's going ound. Such a machine is mentioned by the Marquis of *Worceſter* in his *Century* of inventions N° 56 in the following words. 'To provide and make that all the weights of the deſcending ſide of a wheel, ſhall be perpetually farther from the center than thoſe of the mounting ſide; and yet equal in number and heft to the one ſide as well as the other; a moſt incredible thing if not ſeen; but tried before the late king (of bleſſed memory) in the *Tower* by my directions, two extraordinary Ambaſſadors accompanying his Majeſty, and the Duke of *Richmond* and Duke of *Hamilton*, with moſt of the court attending him. The wheel was 14 foot over, and had 40 weights of 50 pounds a piece. Sir *William Balfore*, then lieutenant of the *Tower*, can juſtify it with ſeveral others. They all ſaw, that no ſooner theſe great weights paſſed the diameter line of the lower ſide, but they hung a foot farther from the center; nor no ſooner paſſed the diameter line of the upper ſide, but they hung a foot nearer. Be pleaſed to judge the conſequence.'

Now the conſequence of this and ſuch like machines, is nohing leſs than a perpetual motion; and the fallacy is this. The elocity of any weight is not the line, which it deſcribes in general, but the height it riſes up to or falls from, with reſpect to s diſtance from the center of the earth: So that when the weight Fig. 19. deſcribes the arch A*a*, its velocity is the line A C, hich ſhews the perpendicular deſcent (or meaſures how much it come nearer the center of the earth) and likewiſe the line B C enotes the velocity of the line B, or the height that it riſes to, hen it aſcends in any of the arches B*b*, inſtead of the arch BD:

So that in this case, whether the weight B in its ascent be brought nearer the center or not, it loses no velocity, which it should do, in order to be raised up by the weight A; nay, the weight in rising nearer the center of a wheel may not only not lose of its velocity, but be made to gain velocity, in proportion to that of its counterpoising weights, that descend in the circumference of the opposite side of the wheel; for, if we consider two radii of the wheel, as represented by Fig. 21. one of which is horizontal and the other (fastened to and moving with it) inclined under the horizon in an angle of 60 degrees, and by the descent of the end B of the radius BC, the radius CD by its motion causes the weight at D to rise up the line *p*P, which is in a plane that stops the said weight from rising in the curve DA, that weight will gain velocity, and in the beginning of its rise, it will have twice the velocity of the weight at B; and consequently, instead of being raised, will overpoise, if it be equal to the last mentioned weight; and this velocity will be so much the greater, in proportion as the angle ACD is greater, or as the plane P*p* (along which the weight D must rise) is nearer to the center: It is true if the weight at B Fig. 19. could by any means be lifted up to β, and move in the arch β*b*, the end would be answered; because then the velocity would be diminished and become βC.

Experiment. Take the leaver BCD (as in Fig. 21.) whose *brachia* are equal in length, inclined in an angle of 120 degrees at C and moveable about that point as its center: In this case a weight of two pounds, suspended at the end B of the horizontal part of the leaver, will keep *in æquilibrio*, a weight of four pounds suspended at the end D: But if a weight of 1 pound be laid upon the end D of the leaver; so that in the motion of D along the arch *p*A, this weight is made to rise up against the plane P*p* (which equally divides the line AC equal to CB) the said weight will keep in *æquilibrio* two pounds at B, as having twice its velocity, when the leaver begins to move. This will be evident, if you let the weight 4 hang at D, while the weight 1 lies above it: For, if you then move the leaver, the weight 1 will rise 4 times as fast as the weight 4.

A Method for Rowing Men of War in a Calm; *by M.* Du Quet, *together with an Experiment and Remarks thereon; by M.* De Chazelles. Phil. Transf. N° 369. p. 239.

TO perfect the art of navigation, two things seem principally wanting; *viz.* an easy method for finding the longitude at sea; and a way to give a vessel its course, when there is no wind stirring.

M. Du

M. *Du Quet* flatters himſelf to have found the laſt and hopes to make it appear, by reaſon and experiment, that a man of war may make a league an hour in a calm, by means of revolving oars, which are eaſily applied to the ſides of a ſhip, without occaſioning any incumbrance; as appears by the following account, after delivering his notion of the motion of bodies in fluids.

A body ſwims upon water, when it weighs leſs than the volume of water, whoſe place it takes up; and it ſinks more or leſs in the water, only in proportion as its volume is more or leſs increaſed.

A body lying in ſtill water is in *æquilibrio*, as it were; the leaſt effort gives it motion and makes it loſe that *æquilibrium*: If the effort be continued, tho' ever ſo little, the motion it communicates will be very ſenſible: How great ſoever the weight of the body be, when once it is in motion, it will always continue ſo, if nothing hinder it.

Upon theſe principles M. *Du Quet* conſiders the motions a veſſel receives by means of oars, and the application of hands that ſet it a-going: The *impetus* of the hand, applied at one end of the oar, and the reſiſtance the water makes againſt the other end, are both impreſſed upon the point, where the oar reſts upon the veſſel: This point is like the *fulcrum* of the common leaver, which always bears the ſum of the weights at both ends, beſides the weight of the leaver itſelf: So that the greater the effort is at one end of the oar, and the reſiſtance at the other end; ſo much the greater is the impreſſion, which the point or *fulcrum* receives, in order to its being put into motion: A galley with two oars only, would go as faſt as it does with the uſual number; provided the ſame number of hands were applied with equal vigour to the two oars; and the oars were ſtrong and broad enough to make the neceſſary reſiſtance; becauſe then the *fulcrum* of the two oars would receive as much impreſſion, as all the *fulcra* of the common oars taken together.

This conſideration put M. *Du Quet* at firſt upon contriving a way, how to apply a great number of hands to the common inclined oars; but after ſeveral trials he threw them aſide and made uſe of perpendicular oars; becauſe the firſt do only ſkim the water, and (when the ſea is rough and the waves run high) they oftentimes do not take water; and ſo become uſeleſs: For, in this caſe, the rowers are tript up for want of meeting a reſiſtance.

This inconveniency is avoided by the revolving oars; because they take the water perpendicularly and enter far enough not to miſs it; and if the water ſhould happen to evade the ſtroke, the rowers would not be ſo incommoded; becauſe they would be ſupported at every vibration, which is only of three foot: Beſides, in the uſe of inclined oars, more than half the time is loſt, in raiſing and recovering the oar, before they give the ſtroke; which makes the veſſel move by jerks and fits; ſo that the people aboard feel, as it were, every ſtroke of the oars when they play; whereas, the revolving oars always move equally, and ſucceed each other without loſs of time; which makes the veſſel move uniformly, without affecting thoſe who are aboard.

It is likewiſe to be obſerved, that a galley built on purpoſe for the uſe of inclined oars, would not be ſo proper as another veſſel built for perpendicular oars; becauſe the galley has a conſiderable length and but little height above the water.

M. *Du Quet*, having propoſed this invention to the court of *France*, was ſent to *Havre de Grace*, to make a trial, which had the approbation of the intendant, who made his report, that the officers at firſt objected to the invention; but as for his own part, the more particularly he conſidered it, the more he was convinced of its uſefullneſs. M. *Du Quet* was afterwards ſent to *Marſeilles*, where he made ſeveral trials on board a galley, the ſwiftneſs of which was compared with that of another galley, equipped as uſual. M. *De Chazelles*, a member of the *Royal Academy of Sciences* and engineer of the king's galleys, had orders to make his obſervations and ſend them to court; a copy of his report, is as follows.

An experiment of the ſwiftneſs of a galley, with perpendicular revolving oars, invented by M. *Du Quet*, compared with that of a common galley, made at *Marſeilles*, &c.

At 10^h $3'$ in the morning, the *ſuperbe* galley quitted her ſtation over againſt the *Auguſtins*, in order to fall down to the *chain*.

At 10^h $11'$ ſhe came to the *chain*.

At 10^h $6'$ the machine galley quitted her ſtation at the innermoſt part of the harbour; ſhe had three machines on each ſide.

10^h $13'$ ſhe came to the *chain*.

10^h $19'$ the two gallies abreaſt. Both row with their whole crews.

$10^h\ 25'$. The *superbe* passes; and then rows only with the hinder part of her crew.

10 27. The machine galley passes.

10 28. Both row with their whole crews.

10 30. The *superbe* galley passes; and then rows only with the fore-part of her crew.

10 32. The machine galley passes; upon which the *superbe* galley claps on more oars, till such time as she has acquired the same velocity with the machine galley; and it appeared, that with seven or eight oars less than her complement on each side, she kept up with the machine galley, making about 200 rowers, which was the number of the machine galley's crew: There was a little wind a-head, which retarded the *superbe* something more than it did the machine galley; because the *superbe* had her masts and yard-arms standing, and the other not.

10 43. Came to the moorings of the isles. The sign given for turning.

10 47. The *superbe* was come about. It appeared that the machine galley was considerably quicker in turning than the *superbe*.

10 30. They came again into port.

By this it appears, that the machine galley has a considerable advantage over the common one, in quitting her station and acquiring her first motion: For, in seven minutes she ran the whole length of the port; having quitted her station by means of her oars, without towing herself by her moorings; which is what another galley would not have effected but very slowly: And the *superbe* galley after she had moved from her station, was eight minutes in going a less distance than the length of the port.

But if we consider that the experiment was made without the harbour, it seems to prove that the common galley had the advantage over the machine galley, tho' the number of hands be equal: For, with eight oars less than her complement on each side, she kept up with the machine galley, notwithstanding the greater resistance of the wind against her masts: However, if we consider, that the crew of the *superbe* was a great deal better than that of the machine galley; that the *superbe* is acknowledged to be one of the best sailors the king has; whereas that which had the machines, is an old decayed galley, and reckoned a very bad sailor; besides, that the crew of the *superbe* are much better acquainted with the common oar, than

than the others are with the new way of rowing; and that in the common galley there is no improvement to be made, either with respect to the proportion of the oars, their length, the breadth of the pallets, the height of the point of rest, &c. or with respect to the construction of the vessel; whereas in the machine there are several things to be improved and alter'd in the oars, the hand-spikes, and in disposing the men to the best advantage.

These things considered, it seems reasonable to believe, that a vessel with the machines might go faster than one with the common oars; because the loss of time is avoided, which happens in the ordinary way of rowing.

This experiment, however defective it be, for the above-mentioned reasons, will prove, that the velocity is greater in this way of rowing than in the other, when the circumstances on both sides are equal: For, by his journal, M. *De Chazelles* finds, that the *Patronne*, in company with 14 other galleys, left the port of *Marseilles* at 50 min. after 3, and rowing all in a calm, came to the isles at 4^h 23′ which made 33′ in going from the *chain* to the isles. But the machine galley made the same way with 200 men, in 30′, having left the *chain* at 10^h 13′; and arrived at the isles at 10^h 43′, tho' there was some wind a-head.

This experiment shews, that the *impetus* does not depend on the number of oars, but the number of men.

A vessel charged with revolving oars will go as fast in a calm with 100 men, as it would do if tow'd by a galley of 200 men; because there will be one galley less to draw along.

Another memoir of M. *De Chazelles*, concerning the usefullness of perpendicular revolving oars, invented by M. *Du Quet*, is as follows.

The experiment made with the new machine, tho' defective by reason of the difference there was with respect both to the crew and the vessels, does still leave room to expect a considerable advantage from this invention, in giving the ship way; For, tho' the common galley should keep up with the machine galley at their first setting out with equal number of hands; it is evident, the machine galley will get the better at long run; when the others crew are so fatigued, as to be obliged to row by turns. For here the men will hold out a longer time; their action not being so great nor so violent: Besides, having only 200 men employed, and being equally mann'd with the other galley, fresh hands may be supplied, and so they will continue

to go at the same rate: For, in case of need the marines may be employed in this service; which they will perform with as little trouble, as they work at the capstane.

The reason of this encrease of velocity plainly appears, if we consider the difference between the common way of rowing and that by perpendicular oars: The last is performed by an uninterrupted application of force, in the same direction; the other acts by jerks: And of the three parts of action that are employed, in order to give the strokes; one in raising the oar out of the water, the second in advancing the hands forwards, and the third in pressing against the water; only the last turns to account; and that still loses something of its efficacy; for, the crew by their falling back all together, make the vessel plunge and render its motion oblique, which contributes very much to its decay.

These are not the only defects of the common oars: For, in order to augment their force, their number is to be increased; and consequently, the vessel must have a greater length; by which means it is rendered weaker and less able to resist the force of the sea: Besides, the vessel must be low built and uncovered (and so more exposed to the beating in of the waves) by reason they are obliged to proportion the length of the oar to the strength and size of the men: And tho' the crew should be under some cover, as they are in a galeass; an opening must be left for the oars to play, by which the waves may beat in.

Both these inconveniencies are avoided by the perpendicular oars; because the addition of force may be obtained; by only applying more hands to the machine; so that with two or three machines on a side, there will be more or less force, in proportion to the number of men employed, and the length of the vessel may be lessened at discretion: And to guard against the sea, another deck may be made, shut close on all sides, even where the axis of the machine passes thro'.

The chief objections against this invention seem sufficiently obviated by M. *Du Quet*'s memoir: But, tho' the whole of what is objected should, indeed, prove, that a vessel made for sailing, as the common galley, would be incumbred with the machines in such a manner, as to make the use of sails impracticable; yet if it still hold true, that she will move faster, as appears both by reason and fact, it must be allowed, that a vessel might be so commodiously constructed to carry these machines,

machines, as to go as faſt as a galley in a calm, and better endure the weather when under ſail.

Such a veſſel would have ſeveral advantages over a galley both in ſailing and in fighting; not to mention the conveniencies of lodging the crew: She may put off to ſea any where, and thereby avoid the dangers attending the coaſt-winds, which galleys find to be a-head as ſoon as they have doubled certain capes; and ſo they find themſelves between two winds, which there would be no danger of, farther out at ſea. With reſpect to fighting ſhe may mount cannon fore and aft and on each ſide; and even mortar-pieces. In time of battle ſhe would be of conſiderable uſe; for, ſhe would take and maintain her poſt without aſſiſtance, either at the head or the rear of the enemy's line and there make uſe of her bombs: Beſides the advantage of towing off other veſſels from their danger in a calm, and of boarding or making off from the enemy: And this holds in ſhips of any rate; provided the length of the oars, the breadth of the pallets and the ſtrength of the hand-ſpikes be proportionable: And the moving force will always be in proportion to the ſtrength and number of the men employed, and not to the number of machines, as in the common oars, which likewiſe are impracticable in ſhips above the fourth rate, by reaſon of their great length, which will be diſproportionate to the ordinary bulk of a man.

By this means the crew will be free from the fatigue of towing, and the veſſel will move incomparably faſter than if it was tow'd; becauſe the chaloups which tow are ſubject to the inconveniencies of the common way of towing, by loſing $\frac{2}{3}$ of the time; and beſides, they cannot act all together: And the veſſel that is tow'd, pulling them back after the oar has made its ſtroke, they have ſo much of the ſpace to regain by the next ſtroke: Beſides, the cable by which they tow, ſinking into the water by its own gravity the reſiſtance the water makes to its return, is to be overbalanced; all which circumſtances together conſiderably diminiſh the towing force.

Beſides, this invention is not ſuch as is deſtructive to mankind, and becomes uſeleſs to the nation that firſt puts it in practice, when generally known; on the contrary it may be greatly advantageous to the inventors at the beginning and every where ſerviceable on ſeveral occaſions, when it is put in practice by thoſe who uſe the ſea.

M. De

M. *De Chazelles* might have added, that the chaloups that
ow, are in cloſe fight liable to be ſunk by the enemy's cannon
nd are expoſed to the waves, by their having ſo little height
bove water.

The chief advantage, and which includes all the reſt, in
oarding or getting clear of the enemy, by means of the new
ars, is, that let a veſſel crowd as much ſail as poſſible, the
erpendicular oars are always capable of increaſing her ſwift-
eſs; becauſe the rowers have only a motion of three foot to
ake one way; and as much the contrary way, in order to
ake the oars deſcribe 54 foot ſpace in the water; and that
otion of ſix foot might be performed in two ſeconds of time,
the oars met with no reſiſtance; conſequently, the veſſel
uſt run 54 foot in two ſeconds, that is, about ſix leagues an
our, before thoſe revolving oars be unſerviceable; for, then
e veſſel would go as faſt as the oars could poſſibly move with
diameter of 18 foot; and if it was neceſſary to make them
ove faſter, it is only lengthening out their diameter; and
ey would move ſo much the faſter, without obliging the
owers to increaſe their own motion.

M. *Arnoult* was ordered to examine the new oars; and he
ade his report to the court, that the officers of the galleys
ound, that they interfered with the uſe of the ſails in a gal-
y; but that they might be of uſe in other veſſels and bomb-
etches; in conſequence of which M. *Du Quet* was ſent to
oulon to make the experiment on board a bomb-ketch.

At the time when the experiment was made, M. *De Vauvre*
nd the officers of the marine were at ſea, and only ſome offi-
ers of the port were preſent, who ſent a verbal proceſs to the
ourt, that was given to a general officer to examine and make
report of it; the reſult of which was, that this invention
ught to be put in practice.

An Account of the ſtocking of the River Mene *with* Oiſters; *by Mr.* Rowlands. Phil. Tranſ. N° 369. p. 250.

THE river of *Mene*, that divides *Angleſey* from *Carnarvonſhire*, has at preſent, *viz.* in 1721, the bottom of its
hannel for ſome miles in length, all bedded with good oiſters,
ſuch plenty, that in the ſeaſon, ſeveral boats are daily em-
loyed to dredge them up; and have done ſo theſe 8 or 9
ears laſt paſt: What is obſervable is, that about 24 years ago,
here were none to be found on that bottom; but that a Gen-
leman about that time, cauſed 3 or 400 large oiſters to be

dropp'd into the channel, just under his land; from the spat or seed of which, it is very probable (the flux and reflux of the tides dispersing it) all the bottom at length, where small stones and a large clutch received the sperm, became covered with oisters: And what favours this conjecture, *viz.* that they are a brood of oisters begun at that time is, that at the first finding, they appeared young and small, but have since yearly increased in bulk and plenty, tho' vast quantities of them have been taken up.

The Longitude of Buenos Aires, *determined from an Observation made there; by* F. Feuillèe; *by Dr.* Halley. Phil. Transf. N° 370. p. 2.

DR. *Halley* had, as occasion offered, collected such celestial observations, as might be of use to determine the longitudes of places on the sea-coast of the world, in order to get as near as possible the outline or true figure of the earth, without which our geography of the inlands must necessarily be very insufficient: The Memoirs of the *Royal Academy* of *Paris* afford a good number of observations of this kind, and among the rest there is one made at *Buenos Aires* on the river of *Plate* in *South America* by F. *Feuillèe* in his voyage to *Peru*; who, in the Memoirs for the year 1711 is said to have observed at that place on the 19. of *August* 1708, the immersion of the star in the southern foot of *Virgo* (marked λ by *Bayer*) behind the obscure limb of the moon: Dr. *Halley*, being desirous to see what longitude might be deduced from this observation, soon found that both the day and star were mistaken: For, that λ of *Virgo* was then nearly in 3° of *Scorpio*, and that the moon would not be there till the next day, *viz.* *August* 20; and the Lat. of λ being about ½ a degree north, the moon at that Long. would be about 3° more southerly than the star; and consequently, far from eclipsing it: For, that at that time the descending node was in the very beginning of *Libra*. Hence he concluded, it must be some other star F. *Feuillèe* observed eclipsed by the moon: The day was certainly the 20. and not the 19. of *August*, as was evident by the place of the moon; but as to the star, it was neither in the *Tychonic* catalogue nor in that more copious one of Mr. *Flamsteed*; but turning over *Hevelius*'s catalogue, the Dr. found a star whose situation agreed with the observation, and was undoubtedly the star, that was observed to immerge behind the moon: The place M. *Hevelius* gives it, allowing the precession

preceſſion of the equinox, was then 1° 58′ 40″ of *Scorpio*, with 2° 51′ and ¼ S. Lat. In order to aſcertain the place of the ſtar, the Dr. on the 21. and 24. of *December* 1721, got ſuch obſervations by help of the circumjacent ſtars, that he was aſſured, the place of the ſtar (which is a fair ſtar of the fifth magnitude) was at that time in 1° 58′ 40″ of *Scorpio* with 2° 54′ and ¼ S. Lat. being upwards of 2′ in Long. and 3′ in Lat. more than *Hevelius* makes it. The hour of this occultation is ſet down preciſely 7ʰ 5′ 38″ at *Buenos Aires*, the Lat. of the place being 34° 35′ ſouth: Whence the altitude of the moon at that place was then 42° 48′ and the parallactic angle 76° 38′ and the parallax in Long. 40′ 11″ to the weſt, and in Lat. 9′ 33″ to the north. So that the moon's obſerved place, corrected by the parallax, was in 2° 28′ 4″ of *Scorpio* with 2° 52′ and ½ S. Lat. To this place (by the *calculus* of thoſe numbers the Dr. fitted to Sir *Iſaac Newton*'s theory of the moon) the moon will be found to have arrived *Auguſt* 2/20 at 10ʰ 57′ 36″ apparent time at *London*. But at *Buenos Aires* it was then computed but 7ʰ 5′ 38″ whence the difference of longitude reſulting from this obſervation is 3ʰ 52′ or 58 degrees, by how much *Buenos Aires* is more weſterly than *London*. The Dr. twice repeated the calculation to avoid error; and by comparing his chart of the variation with the Long. thus found, it appears, that in this caſe a ſhip at ſea uſing thoſe tables and that chart, would by an obſervation of this occultation have fallen with greater exactneſs on the coaſt of *America* than can be pretended to be done by any reckoning.

A Deſcription of an Engine *to raiſe Water by the help of* Quickſilver, *invented by Mr.* Joſhua Haſkins *and improved by Dr.* Deſaguliers. Phil. Tranſ. N° 370. p. 5.

MR. *Haskins* finding, that all hydraulic engines, working with pumps, loſe a great deal of water (always giving leſs than the number of ſtrokes ſhould give according to the contents of the barrels) and that when the piſtons are new leathered to prevent that loſs, the friction is much increaſed and the engines are ſubject to jerks, which in large works often diſorder an engine for a great while, by breaking ſome of the parts; contrived a new way of raiſing water without any friction of ſolids; making uſe of quickſilver inſtead of leather, to keep the air or water from ſlipping by the ſides of the piſtons in the barrels where they work; hoping thereby to prevent all the aboveſaid inconveniencies; and alſo to have

water-engines less liable to be out of order than any hitherto made.

The first experiment Mr. *Haskins* made with an engine set up at Dr. *Desaguliers*'s house, which the Dr. repeated before the *Royal Society* in a model; but by the bad contrivance of the parts, it did not raise near the quantity of water the invention is capable of; the description of the said engine is as follows,

dddd Fig. 1. Plate V. represents a *lignum vitæ* plug or piston (which Mr. *Haskins* called a plunger) about six foot long, made heavy enough with lead at top to sink into mercury, which is previously poured into the barrel D 1 D 2 up to *mm*; the chain E 1 E 2, joined to the piston and the power that moves it, being let down till the piston comes to D 2, the mercury rises to the same height in the barrel and in the receiver R (which it fills) namely to *nn*, as appears in the Fig. then drawing up the piston till its bottom is come to *mm*, the mercury coming out of the receiver down to *oo* makes a *vacuum* and the weight of the atmosphere causes the water to rise up thro' the sucking pipe A 1 A 2 and the valve V into the receiver, where the mercury was before. Upon letting down the piston again, the mercury rises into the receiver and drives up the water thro' the elbow B, the forcing valve *u*, and so up the forcing pipe *a* 2 *a* 1: But when once the forcing pipe (which here was 46 foot high) is full, before any mercury can enter into the receiver and force any water out at the top of the pipe *a* 1, the mercury between the piston and barrel must rise up to *qq* near 3 foot and ½ above the bottom of the receiver, and as it continues to rise up to *pp*, the water is thrown out with a velocity, proportionable to the height that the mercury is raised above the 14th part of the height of the water. Now tho' the friction of solids is here avoided, it is plain, that the mercury must move from *mm* to *qq* without raising any water, and that it can only force in going from *qq* to *pp* and only suck in falling from *oo* to *mm*; and unless the piston be stopped a little while when at lowest, the water will not have time to run out: So likewise the piston must be stopped, when at highest, that the receiver may have time to fill.

Mr. *Haskins* likewise proposed another way, represented in Fig. 2. where the same letters denote the same parts, only here the barrel is moveable by the two chains E 1 E 2; and instead of a solid piston, the hollow cylinder C 1 *cc* is fixed, and the mercury moving up and down in the lower part thereof, sucks and

and forces the water thro' the elbow. The figure represents the engine sucking, by means of the mercury hanging from *oo* to *mm*, in order to force, before any water can be driven out, the mercury in the inner cylinder must descend from *oo* to *mm* and rise up to *pp* between that cylinder and the barrel; so that here likewise a great deal of time is lost; besides the great quantity of mercury us'd, which is very expensive; because so much mercury is mov'd every stroke, as the water rais'd.

These difficulties very much puzzl'd Mr. *Haskins*, and quite discourag'd some other persons that had got the secret of the invention: But Dr. *Desaguliers* considering the matter a little, tho' he had not time to contrive an engine for it, told him, that a little mercury might be made to raise a large quantity of water, and that there would not be such a loss of time as in his engines. In a little time Mr. *Haskins* found out the contrivance represented in Fig. 4. and afterwards that of Fig. 3. which last was what the Dr. had thought of; and both these were also found out by Mr. *William Ureem*, an excellent mechanic.

In Fig. 3. the barrel is mov'd as in Fig. 2. but the plug *ddd* taking up a great deal of space, there is occasion for no more mercury than what will make a concave cylinder or shell up to *pp* between the barrel D 1 D 2; and the hanging cylin- C 1 C 2 *cc*, when the stroke is made for forcing; and a concave cylinder between the plug and C 1 C 2 *cc*, when the suction is made. The Dr. gave Mr. *Haskins* the proportions for an engine this way, of which he made a draught.

In Fig. 4. the barrel with a third cylinder *dddd* instead of the plug of Fig. 3. is lifted up and down every stroke, and the water passes thro' *dddd*, the mercury making a shell sometimes between the middle and inner cylinder, as in the suction; and sometimes between the barrel and the middle cylinder, as in the forcing stroke.

Mr. *Haskins* had contriv'd such an engine, as is represented by Fig. 4. having bespoke the several parts before he died, the pieces of which the Dr. afterwards made use of; and now the whole put together with some alterations, form the engine, represented by Fig. 5. as set up at the Dr's house at *Westminster*; and by the force of one man raises a hogshead of water in little more than a minute and a half to the height of 27 foot: All the fault of the engine Fig. 5. is, that the pendulum handle F*f* is too long, and that the bottom of the middle cylinder

C

C should be exactly in the middle of the height to which the water is to be rais'd, supposing there are three copper cylinders as here represented; and likewise if the barrel D 1 D 2 work'd under the forcing pipe, the lift would be the easier: The engine with this little alteration is represented in Fig. 6.

The sucking and forcing pipe and valves are mark'd with the same letters, as in the other figures; and the chains E 1 E 2 must be suppos'd to hang from such pullies, and to be mov'd by such a pendulum as in Fig, 5. The barrel D 1 D 2 (otherways call'd the outer cylinder, and represented by the same letters in Fig. 7.) has another cylinder within it (call'd the inner cylinder or plug as *d d d d* Fig. 7.) between which two cylinders a certain quantity of mercury is pour'd in, and the hanging cylinder C coming down into the mercury, a stroke of 13 inches may be made by the motion of the barrel, which in going down sucks by causing a *vacuum* in C, and in going up forces the water out of the top of the forcing pipe, performing the office of a common piston; only that instead of leather to make it tight to the cylinder C, there is always a thin shell of quicksilver, either between the middle cylinder C and the inner one *d d d d*, Fig. 7, as happens when the suction is made, or between the middle and outer cylinder, as happens in lifting up the barrel to force: In the suction, the mercury is higher in the inner shell than in the outer, by a height equal to a little more than $\frac{1}{14}$ part of the height of the barrel above the water to be raised; and in forcing, it is higher in the outer shell than in the inner by a little more than $\frac{1}{14}$ of the height of the column of water to be forced: And therefore, if the water be not requir'd to be rais'd above 64 foot, the barrel should move so, as to make the middle of its stroke at the height of 30 foot, or at the middle of the way from the water to be rais'd, to the delivery at top.

Fig. 7. drawn by a larger scale, represents the three cylinders, which are here made of copper in their just proportions; whose lengths, diameters within and without, and thickness are as follows.

Outer

	Outer cylinder or barrel D 1 D 2	Middle or hanging cylinder, C 1 C 2 *cc* in which the ſtroke is made	Inner cylinder or plug *dddd* cloſ'd at top by a cap and movingup and down with the barrel to which it is join'd at bottom,
	Inches	Inches	Inches
Length	30	29, 0	31, 2
Diameter within	6, 74	6, 35	6, 03
Thickneſs	0, 10	0, 08	0, 13
Diameter without	6, 24	6, 51	6, 29

Here B B repreſents part of the elbow of Fig. 5. or of the forcing pipe of Fig. 4. But as the ſpaces between the cylinder are ſo ſmall as not to be viſible, even in a large draught made by a ſcale; the Dr. has here given three more draughts of the three cylinders, where the height is agreeable to the ſcale of the ſeventh figure, but the diameters of the middle and inner cylinders are made leſs than they are in the engine, to make the intermediate ſpace (where the mercury riſes and falls) viſible; and the cylinders themſelves are repreſented by ſingle lines.

The quantity of mercury us'd in this engine is 36 pounds and ½, which being pour'd in between the outer and inner cylinder riſes up to the height of 16 inches.

When the barrel is pull'd up (as in Fig. 9) ſo as to have the middle cylinder within an inch of the bottom of the barrel; the mercury on both ſides the middle cylinder will riſe up to the height of 23,1 inches, that is, about two inches below the cup D 1, to the line *qq*.

When the barrel is going down to fill the ſucking pipe and middle cylinder C, the mercury in the inner ſhell will be 25 inches high, and only 13 in the outer ſhell (Fig. 9.) where the ſhaded part repreſents the mercury.

At the end of the ſucking ſtroke the mercury is up to the top of the inner cylinder, and ſcarce an inch in the outer ſhell, Fig. 8.

In

In raising the piston from forcing to sucking, the first 1 inch and $\frac{1}{4}$ drives the mercury out of the inner shell and raises it in the outer shell 13,28 inches.

The depth of an inch of water in the middle cylinder above the inner one or plug is equal to a space in the outer shell of 13,28 inches; and $\frac{1}{4}$ of an inch is equal to the same height in the inner shell.

Therefore when the mercury is equally high in both shells, a motion of $\frac{1}{4}$ of an inch of the barrel will charge for suction; that is, upon letting down the barrel only $\frac{1}{4}$ of an inch, the pressure of the atmosphere in the outer shell will raise the mercury in the inner one 13,28 inches, at the same time that it pushes up the water from the well 13 foot and $\frac{1}{2}$ high into the sucking pipe; and when all the pipes are full, if the mercury be equally high in both shells, upon raising the barrel one inch, the mercury will rise 13,28 inches in the outer shell; which the Dr. calls charging for forcing; because in continuing to raise the barrel, the forcing valve immediately rises and the water comes out at top, during the rest of the stroke, which is 12 inches, and delivers 1,6 gallon of water, wine measure.

Fig. 10 represents the forcing stroke half way up, with the mercury 17 inches in the outer shell, four inches in the inner, and the whole space at bottom under the middle cylinder seven inches.

From this it appears, that in the whole stroke of 13 inches in length, there is only $\frac{1}{4}$ of an inch lost to charge for suction; and in the next stroke, which is likewise of 13 inches, there is only one inch lost to charge for forcing: So that in a motion of 26 inches, there is but one inch and $\frac{1}{4}$, or about $\frac{1}{20}$ part ineffectual: But this is owing to the too large space of the outer shell, which contains four times more than the inner one, because the cylinders were only hammer'd and not turn'd; for, if the outer space had been no bigger than the inner, then $\frac{1}{4}$ of an inch of the stroke would have charged for forcing: So that only $\frac{1}{2}$ an inch in 26 or $\frac{1}{52}$ part of the whole stroke would have been ineffectual; and in that case $\frac{2}{5}$ of the quantity of mercury, or a little more than 12 pounds would have been sufficient.

There may still less mercury be us'd, if the middle cylinder be made of plate iron, turn'd on the outside and bor'd within; the outer cylinder bor'd and the inner one turn'd: So that

hat if the work be well executed, 8 or 10 pounds of mercury will be ſufficient in this engine; tho' the bore of the middle ylinder or diameter of the column of water, which is rais'd, e of 6,35 inches: If the bore of the ſaid cylinder were but hree inches, leſs than three pounds of mercury would be fficient; and if there were two barrels, leſs than ſix pounds, n order to keep a conſtant ſtream thro' a pipe of almoſt the ame diameter. This will very much leſſen the expence of ercury, which would otherways be an objection againſt this ngine; and by making the inner and outer cylinder of hard ood, as box-wood or *lignum vitæ*, the coſt of the engine may ill be reduced: But if the engine be very large, caſt iron or'd will be proper for the outer cylinder, and caſt iron turn'd n the outſide for the inner cylinder or plug; and hammer'd on bor'd and turn'd for the middle cylinder.

The following objection ſeems at firſt to take off the in-nded advantage of this engine, *viz.* that inſtead of the iction of the leather of a piſton, when we lift up our barrel o force, the reſiſtance the mercury finds to riſe in the outer ell, is at leaſt as great as the friction we avoid: Now that ſiſtance is never greater than the weight of a concave cylin-er of mercury, whoſe height is the greateſt to which the ercury riſes in the ſaid ſhell, and the baſe is the area of the ell itſelf: This weight in the engine is equal to 57,5 pounds; d therefore one would think it greater than the reſiſtance ade by the friction of a piſton: But if it be conſider'd, that the deſcent of the barrel for ſucking, the mercury ſhifts mediately into the inner ſhell, riſing to the ſame height, d ſtill keeping the ſame baſe; the aforeſaid weight of 57,5 ounds helps down the barrel, and facilitates the overcoming e force of the atmoſphere; conſequently the weight of the ercury being balanc'd, is no hindrance, whether you work ith a ſingle or double barrel.

There only then remains the hindrance by loſs of time in e beginning of any ſtroke: But the Dr. has ſhewn that to e but $\frac{1}{12}$ part of the ſtroke. He has found, that the beſt gines, now in uſe, generally loſe near $\frac{1}{5}$ of the water, that ey ought to give, according to their number of ſtrokes: And r. *Henry Beighton*, having a great many times meaſur'd e water, rais'd by engines in mines, found, that ſome engines ſt $\frac{1}{4}$; and none ever loſt leſs than $\frac{1}{5}$ of what they ought to ve, according to the number of the ſtrokes in their pumps, hatever auxiliary powers they were mov'd with.

 There

There is another objection, but scarce worth notice; and that is, that some particles of mercury will mix with the water that is rais'd and make it unwholesome: But no body, that considers specific gravity, will imagine any such thing. However, to satisfy such as might still apprehend it; it is to be observ'd, that none of the water that is rais'd comes near the mercury: For, in the cylinder C and part of the elbow B Fig. 5. there is always above the mercury a certain quantity of water that rises and falls with the barrel, and never goes into the forcing pipe. The same does likewise happen in the engine represented by Fig. 6. For, the water having once run into the cylinder C, all that is rais'd afterwards, comes thro' the forcing valve, without coming down to the mercury.

Provided care be taken to make the barrel with its plug tight, this engine will not want repair in a long time, unless some of the auxiliary powers be out of order, which have no relation to this invention. The numbers given will serve to examine the truth of what the Dr. has asserted concerning the motion of the mercury: And from them one may make tables to serve to proportion these engines for raising any quantity of water to any height, according to the power to be applied.

An Account of the coming off of the Scapula, *and Head of the* Os Humeri *upon a Mortification; by Mr.* Peter Derante. Phil. Trans. N° 370. p. 15.

NOV. 5. 1713. *John Fletcher* on board the *Neptune* of *Liverpool* broke the *radius* and *ulna* of his left arm, and their ends burst out thro' the skin: He was immediately dress'd by the surgeon of the ship with the common astringents and bandages: About five or six days after, Mr. *Derante* was sent for to see the patient; and upon taking off the dressings, he found it black and insensible from his fingers to his shoulder; and therefore advis'd the extirpation thereof immediately; but his surgeon oppos'd it: However Mr. *Derante* scarified it in several places, and very deep, and then applied a warm dressing: Next day the ship put to sea, and the patient was sent to *Waterford* and committed to Mr. *Derante*'s care. As soon as he could get his *apparatus* ready, he cut off his arm as high as possible; afterwards he cauteriz'd the stump, which was perfectly mortified as high as the *acromion*. Next day he perceiv'd the mortification spreading towards the lower angle of the *scapula*; then he rubb'd the edges of the mortification with arm'd probes, dipp'd in a solution of *argent. viv.*

viv. in *aq. fort.* which compleatly anſwer'd his intention: For, from that time the mortification ſpread no farther; next dreſſing he ſcarified and cauteris'd all the mortified part and dreſs'd it *ſecundum artem.* He continued this method for 17 or 18 days; and then the ſloughs began to ſeparate and caſt off daily: Some time after, the *ſcapula* began to part from the *Os humeri* and *clavicula*; and at length came out entirely; the *Os humeri* ſtill adher'd to the *pectoralis* and *latiſſimus dorſi*; but in a little time it alſo ſeparated and came away, without any hemorrhage ſucceeding: He was afterwards oblig'd to cut off part of the *clavicula*, before he could cicatrize the wound, which was ſoon after accompliſh'd.

A great Number of Hydatides *found in the* Abdomen; *by M.* Bartholin Anhorn ab Hartuiſs. Phil. Tranſ. N° 370. p. 17. *Tranſlated from the* Latin.

A Man of 50 years of age, of a cholerico-phlegmatic conſtitution, and ſquare ſtature; not fat but robuſt, of a good appetite, and who went about his ordinary buſineſs very well, and obſerved a pretty good *regimen*, not given to wine or ſpirituous liquors to exceſs, in *Auguſt* 1721 applied to M. *Anhorn* in the caſe of a ſwell'd and tenſe *abdomen*, not unlike an *aſcites*; tho' his urine was laudable, and in a ſufficient quantity, and he had no *œdematous* ſwelling in his legs: his reſpiration was free, tho' his belly increaſed daily in bulk, having begun to ſwell the *June* before; he had the hemorrhoids to a great and ſometimes painful degree; yet without any loſs of ſtrength or appetite.

M. *Anhorn* was at a loſs to determine, whether it were a dry or moiſt tumour: But to ſatisfy the indications, particularly the ſuppreſſion of the hemorrhoids, he adminiſtred chalybeates, mix'd with purgatives, by which the hemorrhoidal flux was immediately promoted, the pain abated and a large quantity of *fœces* with ſome relief to the patient, diſcharged: Afterwards M. *Anhorn*, as *Etmuller* had done in the caſe of a dry dropſy, tho' to no purpoſe, adminiſter'd daily *unc. ſſ. Nitr. præparat.* as alſo *Mercur. Vitæ.* together with the milder and ſtronger diuretics, &c.

The patients *abdomen* increaſing very much in bulk, it was neceſſary without farther delay to proceed to the *paracenteſis*, which was perform'd *Jan.* 14. 1722; but the contents being of a glutinous conſiſtence could not be diſcharged by the *cannula*: ſo that an inciſion muſt be made with a lancet; upon

which there was discharged the first day about six measures of a glutinous matter of a grey dirty colour, thicker than the whites of eggs, together with a large quantity of white globules of different figures, as spherical, triangular, *&c.* of the bigness of filberds, yet without loss of strength: Next day again the incision was enlarged with a lancet, upon which a great number of globules exceeding in bigness pidgeon's eggs, was discharged, together with six measures more of a glutinous matter, and a large piece of a whitish mass resembling a portion of the *omentum*, to which mass several of the said globules adher'd by fibrous filaments.

Upon setting the glutinous matter over the fire, it became hard and white, with a pellicle on it.

And upon pouring on the said matter spirit of vitriol, spirit of *Sal-armoniac*, volatile oil of tartar *per deliquium*, rectified spirits of wine and vinegar, there was nothing other observ'd than a gentle induration thereof, by each of the said liquors, tho' of quite opposite natures.

The globules were of a very white colour, and hard to the touch; upon breaking, there issued out a white liquor, resembling chyle, and included in a membrane or pellicle.

That whitish mass, resembling a portion of the *omentum*, tho' an inch thick, was evidently membranous.

The number of small and large globules was about 7 or 8000.

The 16. day inclusive after the operation, the patient did very well, his belly was soft and smooth, the wound look'd well, his urine, pulse and respiration were laudable; consequently, he had no fever, and he slept sound: For 13 days there was a plentiful discharge of a glutinous inodorous matter, and which at length became serous and in little quantity, but of a strong smell, and turning black a silver catheter put into it, which was ascribed to corrupted hetereogeneous bodies rather than to the *viscera* being affected with a gangrene, because there was no fever. On the 16. day after an injection of a decoction of elecampane, agrimony and mellilot with a tincture of aloes, myrrh and *balsam du commandeur*, a pellicle of a broken globule was discharged, which in its natural state was as big, and probably bigger than a hen's egg.

The patient's appetite and strength were now the chief things to be managed; M. *Anhorn* administer'd vulnerary, alexiterics and comforting medicines, combined together according to the rules of a strengthning diet.

M. *An-*

M. *Anhorn* leaves others to discuss this remarkable phænomenon.

An Account of some Experiments made with the Bile of Persons Dead of the Plague *at* Marseilles, *with what appeared upon the Dissection of the Bodies; as also some Experiments made with the Bile of Persons Dead of other Diseases; by* Dr. Deidier. Phil. Transf. N° 370. p. 20.

Exp. 1. THE human bile, taken from the gall-bladder of the bodies of such as died of the plague at *Marseilles*, has always been found to be of a black and greenish colour: Upon mixing spirit of vitriol with the said bile, it constantly became of a lasting grass green; and upon mixing therewith oil of tartar *per deliquium*, or its alcalious fixed salt, dissolv'd in a sufficient quantity of water, it always became very yellow: These two colours green and yellow have continued for whole months. The said bile has become of a black colour, like ink, but it soon fades, by pouring spirit of nitre thereon.

Exp. 2. The bile taken from the gall-bladder of the bodies of such as died of the plague, having been poured into a wound made on purpose in several dogs, rendered them melancholly, drowsy, without caring to eat: All these animals died in three or four days with the essential marks of the true plague; as bubo's, carbuncles and gangrenous inflammations in the *viscera*, in the same manner as in the human bodies from which the bile was taken.

Exp. 3. A drachm of the said pestiferous bile having been mix'd with two ounces of fountain-water, made lukewarm, and injected in the jugular vein of several dogs, rendered them presently drowsy and killed them in four hours, with gangrenous inflammations, the heart stuffed full of black thick blood, the liver swell'd and the gall-bladder full of green bile.

Exp. 4. The same quantity of bile, injected by the crural vein, in about an hour produced a heaviness in the dogs; they had such a strong loathing of their food, that they would neither eat or drink after the injection was made; they frequently made water, especially if stirred: On the third day there appeared considerable tumours under the *axilla* and their thighs, about three inches from the wound; the wound turned to a gangrene, and the animal usually died on the 4th day, with all the signs of the plague.

Exp.

Exp. 5. A dog, belonging to the hoſpital at the *Mail* in *Marſeilles*, who followed the ſurgeons, when they went to dreſs the patients, uſed greedily to ſwallow the corrupted glands and the dreſſings full of *pus*, taken off the plague-ſores; he lick'd up the blood, he found ſpilt on the ground in the infirmary; and this he did for about three months; and was always well, gay, briſk, full of play and familiar with all comers; into the right crural vein of this dog about a drachm of the peſtiferous bile was injected, mixed with two ounces of warm water.

He died the 4th day, as the others did, with a bubo on the wounded thigh; on which there were likewiſe two carbuncles and the wound was gangren'd. What was remarkable in this dog is, that after the injection, both when alive, and after he was opened, he had a very ſtinking ſmell, which was not obſerved in any of the others: Moreover, there was a conſiderable hemorrhage from the wound, the night before he died, having ſtruggled hard to eſcape out of his confinement.

Exp. 6. *May* 2. having injected into the crural vein of a dog about a drachm of human bile, taken from perſons dead of the plague, mixed with two ounces of water, the creature became preſently drowſy, refus'd his food, and died about the 3d or 4th day after the injection, with all the inward and outward ſigns of the plague that the others had.

Exp. 7. *May* 6. the bile of this dog, dead of the plague, being injected by the crural vein into the blood of another dog, he, preſently after the injection, had convulſive motions all over him, that were followed by a lethargic heavineſs: On the ſecond day after the injection, there appeared a carbuncle upon the great pectoral muſcle on the right ſide: The third day there aroſe a conſiderable bubo on the thigh, and the dog died the ſame day. Upon opening the body the fore-part of the breaſt was found all gangrened under the teguments; and in the inner parts, the *viſcera* were full of black clotted blood; as in all the others: The external ſurface of the lungs was all of a purple colour; the heart was ſwelled as big again as uſual, with the four cavities full of black clotted blood. The dog lived three days after the injection without eating or drinking.

May 10. the bile of this ſecond dog was injected into the crural vein of a third dog, who was thereupon ſeiſed with violent convulſions and various convulſive motions for about half a quarter of an hour: When he recovered from theſe, he appeared dull and ſleepy; he vomited with violent ſtrainings; the vomiting

was

was followed with an hick-up: He eat a little boil'd meat, having fasted a good while before the injection was made; but he vomited it up two hours after he had taken it. He died the third day with the same signs of the plague that the other dogs had.

The state of the bodies of such persons as died of the plague, from which the bile for the abovementioned experiments was taken, is as follows.

Belefleur a soldier, 25 years of age, of a strong robust constitution, having a flattish bubo growing on the hollow of the right groin, died delirious: Upon opening his body his heart was found of an extraordinary bigness, stuffed with black clotted blood; his lungs covered with a livid purple, and adhering a little to the *pleura*; the liver was twice the natural bigness, and stuffed with a thick blood; the gall-bladder was full of a black and greenish bile; the *dura* and *pia mater* seemed by their blackness to have been seised with a gangrenous inflammation; the internal substance of the brain was sprinkled over with an infinite number of small livid spots.

Mary Pisane, 30 years of age, of a sanguine habit, had a bubo under her right arm-pit, with a *delirium* that was followed by a mortal sleepiness: In opening her body, the lungs were in their natural state; the heart was of a prodigious bigness, full of black coagulated blood, with the left auricle livid and gangrenous; the liver, which was much enlarged, was all covered with purple; and the gall-bladder filled with a black and greenish bile.

Peter Moulard, of a tender, feeble constitution, about 14 years of age, had a bubo under the hollow of the right groin, that was very deep and never came well out: He became delirious, with convulsions, in which he died. Upon opening his body, the heart was found twice its natural bigness, containing a black thick blood; his lungs had livid spots here and there; the gall-bladder was full of a black and greenish bile.

Jean Raynaud, a cook, about 25 years of age, of a melancholly temperament, had the whole habit of his body covered with a purple livid colour, and a bubo under his left arm-pit, he died in a delirious phrenzy. Upon opening his body, two abscesses were found, one between the teguments and the left great pectoral muscle, and the other in the breast between the *sternum* and the *mediastinum*; his heart was very large, filled with black thick blood; the right auricle was three inches broad; the left was in its natural state; his lungs were covered with small livid spots, remaining soft and pliant, without any hardness in their substance

ſubſtance; the liver was larger and harder than ordinary, and alſo full of livid purple ſpots; and the like were found in the ſubſtance of the brain, all the veſſels of which were filled with black thick blood.

Jaques Audibert, about 35 years of age, of a melancholly complexion, four months after he had been cured of the plague (the ſign of which was a bubo in the fold of the right groin, which came well to a ſuppuration) was attack'd afreſh with three carbuncles; one in the middle of the arm and the other two in the fore-arm: He had but little fever, with ſome little ſickneſs at ſtomach; but a delirium, ſeiſing him of a ſudden, carried him off. Upon opening his body, his heart was found of a prodigious bigneſs; the right auricle being five inches broad and the left three; a ſmall impoſthume was found upon the body of the *aorta*; the lungs were covered with livid ſpots, and the liver appeared gangrenous; the gall-bladder was of a very black colour; the *duodenum* and *rectum* were inflamed.

Venture Cajole, about 40 years of age, of a melancholly temperament, died on the 3d day without any outward eruption, of a violent fever, with a ſleepineſs: Upon opening the body, the *mediaſtinum* was found torn towards the upper part; the *pericardium* was of a livid colour; the heart larger than in its natural ſtate, by the ſwelling of its ventricles, which were full of black thick blood, as in the other ſubjects; the liver was likewiſe very large and of a livid colour, with a carbuncular puſtule on the ſide of the gall-bladder, which was filled with very black bile.

Margueritte Bachaire, 18 years of age, of a lively vigorous conſtitution, having two carbuncular puſtules on the middle and inſide of the thigh, with a ſharp pain in the head, died delirious. Upon opening her head the integuments of the brain were found of a blackiſh red; the cortical part of a livid red and a few black ſpots upon the medullary part; the heart was of a prodigious ſize, full of thick black blood; the liver was likewiſe very large, and the gall-bladder very full of a black and green bile; there were ſeveral livid ſpots upon the ſurface of the inteſtines.

Louiſe Belingere, 20 years of age, having a bubo in each fold of the groin, died very ſuddenly without any dangerous ſymptoms. Upon opening her body, the heart was found all covered with a livid purple, and much larger than natural, filled with thick black blood, with a *polypus* in each ventricle; the lungs were in their natural ſtate; the liver was of a prodigious bigneſs; and the gall-bladder full of bile of a deep green colour.

Rampeau,

Rampeau, a peasant, about 20 years of age, of a sanguine obust constitution, having had a carbuncular parotid for the pace of eight days, accompanied with a burning fever, was carried to the hospital the 2d of *May*, where he died the 5th. The xternal part of the left side of the lungs was found covered with livid purple; the heart was twice its natural bigness, having carce any blood in the ventricles, each of whose cavities was illed with a large *polypus*, that on the right side having dilated he auricle to the breadth of four inches; the liver also was arger than ordinary, and the gall-bladder full of a black and reen bile.

The abovementioned experiments and the opening the bodies ere made in the hospital at the *Mall*, in the apothecary's shop f the reformed Fathers of *Marseilles*, during the months of *February*, *March*, *April* and *May*.

The following experiments were made at *Montpelier*, in the ospital of St. *Eloy*, during the months of *September*, *October* nd *November*.

A soldier, of about 20 and 25 years of age, of a lively brisk emper, being sick in the hospital of St. *Eloy* of a common malignant fever, died about the 15. day by a defluxion upon his reast: His lungs were found very much inflated, filling all the avity of the breast, and adhering to the *pleura*: Upon observing the bile in the gall-bladder of a bright grass-green colour, it as reserved for the following experiments.

This bile, mixed with four ounces of warm water, was partly njected into the jugular vein of a dog, and a compress, soak'd in he rest of the liquor, was applied to the wound. The dog hereupon appeared heavy and sleepy, and would neither eat nor rink; the third day he eat and drank freely. The compress alling off the fourth day, the wound was diminished one half nd healed up by degrees, and the dog grew perfectly well.

Exp. 8. A peasant of about 50 or 60 years old, of a melancholly temperament, had been near a month in the hospital with n ordinary malignant fever, being alternately delirious and leepy: After his death, the bile was found exceeding thick, and lack as ink, and in great quantity: About a drachm was put nto a wound made for that purpose on the outer part of the right high of a dog, thrusting in pledgets, dipt in the said bile diluted, into the wound: No change appeared in the dog: He was made to swallow some of the said bile, without losing his appeite; and seeing he was like to be well, the wound healed up of tself in 15 days, only by the dog's licking it.

Exp. 9. A pledget, ſoak'd in as much of this bile diluted, as it could take up, was applied to a freſh wound, made in the inſide of the right thigh; the pledget was faſtened within the ſkin by ſome needles: This application produced no conſiderable alteration in the dog; he neither appeared ſleepy nor without appetite; but lick'd his ſore readily enough; and after the pledget was fallen off, the wound healed up as in the foregoing experiment.

Exp. 10. About a drachm of the ſame black bile, taken from the ſame body and mixed with warm water, was injected into the jugular vein of another dog, who was not incommoded thereby, but as briſk as before the injection, only he appeared very thirſty and lapped greedily: Next morning the wound was black and dry, and the dog, becoming ſurly, bit one of the aſſiſtants: The two ligatures made for the injection were taken off, without obſerving any blood to run out: A doſſil was applied, charged with the ordinary digeſtive and kept on by a bandage; and about four hours after the dreſſing the dog was found dead, having lived 23 hours after the injection: Upon opening him, his heart beat ſtill with violence, and upon the ceaſing of the pulſation there was no blood either in the ventricles or auricles. This liquor, crowded together in the large veſſels, appeared of a lively red and very fluid, without any of thoſe concretions, conſtantly obſerved in all the bodies, that died of the plague: Here there appeared neither internal nor external ſigns of the plague.

Exp. 11. An inhabitant of *Montpelier*, about 30 or 35 years of age, very fat and robuſt, of a ſanguine complexion, having had a fall upon the pavement, received a ſimple wound on the upper part of his fore-head on the right ſide: This being neglected, brought on an *eryſipelas* all over his face, which was accompanied with a ſwelling of the left parotid: This appeared and diſappeared thrice from morning to night. The *eryſipelas* came ſuddenly on; the patient grew delirious and died after 15 or 20 days illneſs, reckoning from the fall. Upon opening his body, a quantity of water was found between the ſcull and the *dura mater*: The brain, which was firmer than ordinary, was ſomewhat red, and part of the *pia mater*, covering the hinder part of that *viſcus*, appeared inflamed. There was about ½ a *ſeptier* of water of a yellowiſh colour, ſhed in the cavity of the breaſt; the great right lobe of the lungs was ſomewhat hard on its upper part; the heart had a *polypous* concretion in each ventricle: Two *French* pints of limpid water were likewiſe found in

in the lower belly: All the fat of this subject was yellow: The liver appeared somewhat swell'd, and the gall-bladder almost empty, not having above two drachms of yellow bile therein. The bile of this subject, mixed with two ounces of warm water, was injected into the crural vein of a dog, who eat and drank heartily after the injection, and did not appear at all incommoded: The wound bleeding much, it was filled with restringent powders, kept in by a pledget and a convenient bandage: Twenty four hours after the dressing was taken off, the wound appeared black and dry: The dog licking it, it suppurated the next day, and afterwards became red and well coloured; and the wound was lessened one half in eight days, during which time the dog appeared in perfect health.

Exp. 12. Eight days after the foregoing experiment, the dog that was the subject of it was killed by about $\frac{1}{2}$ a drachm of powder of *Hungarian vitriol*, dissolved in a spoonful of warm water, and injected into the jugular vein: The creature died on the spot with universal convulsions: His heart was found full of grumous blood, reduced to a kind of thick pap, but without any clotts: The bile of this dog was yellow and in small quantity: Not being able to inject it into the crural vein of another dog, because the vein was too small, two compresses were dipp'd in this bile, which were applied and kept under his skin by two wounds, made on purpose; but without any remarkable change. In these two dogs were observed no signs of the plague either internal or external.

The Method of inoculating the Small-pox *in* New England; *by Mr.* Newman. Phil. Transf. N° 370. p. 33.

1. A Couple of incisions is usually made in the arms, where issues are made, but somewhat larger than for them; sometimes in one arm and one leg. 2. Into these bits of lint are put (the patient at the same time turning his face another way, and guarding his nostrils) dipt in some of the variolous matter, taken in a phial, from the pustules of one that has the small-pox of the more laudable sort, and just turning upon him, and so covered down with a plaister of *diachylon*. 3. The variolous matter, from such as have the inoculated small-pox, is altogether as agreeable and effectual, as any other; as also that, taken from such as have the confluent sort. 4. In 24 hours after, the lint is taken off, and the sores are dress'd once or twice every 24 hours, with warmed cabbage leaves. 5. The patient continues to go about his affairs, as at other times, only he must not expose him-

ſelf to the injuries of the weather, if any ways tempeſtuous. 6. About the 7th day the patient feels the uſual ſymptoms of the ſmall pox coming upon him; and he is now managed, as in a common putrid fever; if he cannot hold up, he goes to bed; if his head ach too much, the common poultice is put to his feet; if he be very ſick at the ſtomach, he has a gentle vomit; nay, commonly theſe things are almoſt done of courſe, whether the patient want them or not, and the ſooner, it is thought the better; if the fever be too high, in ſome conſtitutions, a little blood is taken away; and finally to haſten the eruption, a couple of bliſters is applied. 7. On or about the third day from the decumbiture the eruption begins: The number of the puſtules is not alike in all; in ſome they are very few; in others they amount to 100; nay, in many to ſeveral hundreds; and frequently to more than by the accounts from the *Levant* is uſual there. 8. After the eruption, all illneſs vaniſhes; except, perhaps, a little of the vapours in thoſe that are troubled with them: And there is nothing other neceſſary than to keep warm, drink proper teas, eat gruel, milk pottage, panada, bread, butter and almoſt any thing equally ſimple and innocent. 9. Ordinarily the patient ſits up every day, entertains his friends, nay, ventures on a glaſs of wine; if he be too intent upon hard reading and ſtudy, he muſt be taken off it. 10. Sometimes tho' the patient be eaſy enough on other accounts, yet he cannot ſleep for ſeveral nights together: In this caſe no anodynes or opiates are given; becauſe it is found, that ſuch, as have taken theſe things in the ſmall-pox, are generally peſter'd with biles after their recovery; and ſo they are omitted, ſleep coming of itſelf, as their ſtrength comes on. 11. On the 7th day the puſtules uſually come to maturity; and ſoon after go off, as thoſe of the ſmall-pox in the diſtinct ſort. 12. The patient gets abroad quickly, and is moſt ſenſibly ſtronger, and in better health than he was before. The tranſplantation has been made on women in child-bed, eight or nine days after their delivery; and they have got earlier out of child bed, and been in better circumſtances than ever in their lives before: Thoſe that have had ugly ulcers, long running upon them, have had them healed up: Some very feeble, crazy, conſumptive people have, upon this tranſplantation, grown hearty and got rid of their former maladies. 13. The ſores of the inciſion ſeem to dry a little in three or four days of the feveriſh preparation for eruption; after this there is a plentiful diſcharge at them: The diſcharge may continue a little while after the patient is quite well in other reſpects; but the ſores will ſoon enough dry up of themſelves;

but

but the later, it is thought the better. If they happen to be inflamed, or otherways troublesome, they are presently managed as common sores.

An Account of the Inoculation of the Small-pox *at* Halifax *in* Yorkshire; *by Dr.* Nettleton. Phil. Transf. N° 370. p. 35.

DR. *Nettleton*, having too often found, how little the assistance of art could avail in several cases of the small-pox, was induced to try the method of insition or inoculation, which came so well recommended by several physicians from *Turkey*, and which had also been practised in *London*; the following is an account of its success and of every thing either done or observed in it at *Halifax* in *Yorkshire*.

In *December* 1721, the Dr. first began to put this method in practice, and finding it to succeed beyond expectation in the first instance, he was encouraged to repeat it upon some others; and afterwards several people, seeing with how much ease those got over the distemper, were desirous to have the same done either on themselves or their children; so that there were upwards of 40, who received the small-pox by insition, and who all got well and were afterwards in perfect health, only one patient dying.

What was done by way of preparation, was chiefly purging with rhubarb for children; and sometimes vomiting or bleeding for grown persons; and several had no preparation at all. But the Dr. always found, as far as he was able to judge, that those, whose bodies were well prepared by such proper methods, as their different ages or constitutions seemed to require, had more favourable symptoms than others in like circumstances, where that was omitted.

The method the Dr. always took in the operation, was to make two incisions; one in the arm and another in the opposite leg: It is not material as to raising the distemper, whether the incisions be large or small, but he commonly found, that when they were made pretty large, the quantity of matter discharged afterwards at those places was larger; and the more plentiful that discharge, the more easy the rest of the symptoms generally are, and by this means they are also best secured from any inconveniency, which might follow, after the small-pox are gone off.

At first the Dr. collected some of the matter from the pustules of one, who had the small-pox of the natural sort, into a shell or phial, and infused two or three drops of it into the wound; but finding it to be very troublesome and difficult to get any quantity of

of the matter, and likewise observing, that the smallest quantity imaginable will be sufficient for the purpose, he commonly took small pledgets of cotton and ripping the pustules, when ripe, with the point of a lancet, he rolled the pledgets over them, till they imbib'd some of the moisture; one of these he put on each wound, and cover'd it with any common plaister till next day, when he commonly took away both the cotton and the plaister, leaving the wound to itself, only covering it with a slight linen roller to defend it from the air: He has sometimes rubb'd the pledget only once over the wound, without binding it on, which he found to answer the end as well; and from some other observations he made, he has been surpris'd to see the small pox produced this way, when he was very well assur'd, the quantity of matter, receiv'd into the vessels, could not amount to the hundredth part of a grain.

The persons inoculated were not confined to any regimen, but only to be kept moderately warm; and those, who were grown up, to live very temperate and regular, to keep their minds easy and compos'd, and to use proper means to drive away all fear and concern: Some have been oblig'd from the time of the insition to abstain from flesh and all strong liquors; but he afterwards found, that the insition did not proceed so well, when they were obliged to live too low. Perhaps in warmer climates where they are not so much accustom'd to live on meat, such abstinence may be necessary; but here he found it best to let them eat or drink as usual, tho' something more sparingly, till the fever begin to rise; and then, but not before, such a regimen, as is usual in like cases, is enjoin'd.

The first thing that occur'd after the insition, was the inflammation of the wounds, which commonly happened about the fourth day, when they began to appear very red round about and to grow a little sore and painful; in about two days more they began to digest and run: In some they begin to run sooner, and the quantity discharged is much greater than in others. The Dr. generally found, that in those, who discharged most this way, the fever was more slight and the small pox fewer, tho' he has known some do very well, when these places have only appear'd very red; but have scarce run any thing at all, as it usually happens, when the incision is made so superficial, as not to cut thro' the skin.

About

About the ſeventh day the ſymptoms of the fever begin to ome on, which are the very ſame, that are always obſerv'd in e ſmall-pox of the diſtinct kind, in the natural way: A uick pulſe, great heat and thirſt, pain in the head and back, nd about the region of the ſtomach, vomiting, doſedneſs, artings, and ſometimes convulſions; all were not ſeiz'd with ll theſe ſymptoms, nor in the ſame degree or continuance; me began on the ſeventh day and continued ill without any emiſſion, till after the eleventh; and many not till the eighth r ninth day; and in theſe the fever was more moderate with onſiderable intermiſſions; and ſome have ſcarce had any lneſs at all: During all this time the places of inciſion conued to be very ſore, and ſwell very much; ſo as to appear ry large and deep, and diſcharge a great deal of matter.

On the tenth day the ſmall-pox moſt commonly appear'd, metimes on the ninth, and ſometimes not till the 11th; but e Dr. never found, that any difference of age, conſtitution or ny other cauſe, ever made them vary above one day from the oth. The number of puſtules was very different; in ſome ot above 10 or 20; moſt frequently from 50 to 200; and me have had more than could well be reckoned, but never f the confluent ſort: Their appearances was the ſame with oſe of the diſtinct kind; they commonly came out very ound and florid; and many times roſe as large as any he bſerv'd of the natural ſort, going off with a yellow cruſt or ab as uſual; tho' it ſometimes happened, eſpecially when e ſores diſcharged a very large quantity of matter, that ey were both few in number and did not riſe to any bulk; ut having made their appearance for four or five days, they aſted inſenſibly away.

After the ſmall pox come out, the feveriſh ſymptoms graually abate, and when the eruption is compleated, they ſually ceaſe without any ſecond fever, or any farther trouble n any reſpect.

While the puſtules were riſing, and for ſome time after they ere gone, the ſores continued to ſwell and to run very much; e longer they do ſo the better: But they never fail'd to heal p of themſelves after a certain time.

The Dr. rarely ſaw occaſion for any medicines in the courſe f the diſtemper, only ſometimes when the ſymptoms ran very igh, he gave a gentle anodyne, to be repeated, as occaſion hould require; and once or twice he thought it neceſſary to liſter and to uſe ſuch medicines, as are found to be moſt ſer-

viceable

viceable in the ſmall-pox of the natural ſort. After the puſtules are gone off, the patients have always been purged twice or thrice and ſometimes let blood, which is all that has been uſually done. But tho' the practice may ſeem to be very eaſy, yet it is an affair of ſuch a nature, as to require the utmoſt care; and never to be undertaken without the advice of phyſicians to direct a proper method of preparation before the inſition is made, as well as a juſt regimen afterwards; to watch every ſymptom, and lend nature all proper aſſiſtance, whenever it ſhall be requiſite. Where this is done, it will ſeldom fail of being attended with happy ſucceſs.

In one inſtance or two it has happened, that the ſymptoms in the diſtemper have been worſe than uſual; and ſome few, after the ſmall pox were gone off, have been ſubject to other indiſpoſitions; a particular account of which is, as follows.

A boy of *Halifax*, about a year and a half old, that was inoculated, began to be ill on the eighth day, and had a briſk fever for two days; then the puſtules began to appear, which were but few in number, and roſe very large: The child was ſoon well and continued ſo for about a month; when being carried out and kept a long while in the cold, he fell into a feveriſh diſorder, accompanied with a cough, and was ill for four or five days: After that time it went off, and ever after he was in very good health.

A girl about two years of age, in a family where they had formerly buried three children ſucceſſively of the ſmall-pox, was inoculated: The fever came on about the ſeventh day, and ſhe continued very ill till the tenth, on which day about noon ſhe had a ſtrong convulſive fit. In the evening the ſmall-pox appear'd; and tho' ſhe had more in number than uſual, yet ſhe grew well as ſoon as they were fully come out, and continued ſo ever after.

In a family, where they had four children, none of whom had had the ſmall-pox; the eldeſt was ſeis'd in the natural way with the moſt malignant ſort he ever ſaw, attended with the worſt ſymptoms, inſomuch that he died on the fourth day, all full of purple and livid ſpots: The other three the Dr. inoculated the day before the eldeſt died, after having told the parents, that he could not anſwer for the ſucceſs in caſe they had already catch'd the infection, which would be known, if any of them fell ill before the ſeventh day. Accordingly one of them began on the ſecond day, much after the ſame manner with the eldeſt, and the ſmall pox

appeared

ppeared on the third day, or rather an univerſal redneſs all ver the ſkin, interſpers'd with ſeveral livid ſpots: There ere none of theſe ſpots near the places of inſition, which egan to ſwell a little, as uſual, about the fourth day, and he ſmall pox roſe a little more about thoſe places than lſewhere; but nature was too far oppreſs'd with the violence of the diſtemper, and tho' this girl continued longer han her brother, and was not ſo delirious as he was, yet ſhe ied on the ſeventh day. The Dr. did not reckon this child in he number of thoſe who receiv'd the diſtemper by inoculation; or, he thought there was ſufficient reaſon to conclude, that he had taken the infection before. The other two continued ell till the eighth day, when they both fell ill together: The ſmall pox came out on the tenth, of a very good ſort, ho' more in number than ſome others had; and they both got ery eaſily over the diſtemper, without any indiſpoſition after. t was obſervable, that the elder of theſe children, about the ime of the eruption, had ſeveral ſpots that appeared on him f a deep red colour, very much like thoſe of the other two hildren, which afterwards changed in them to purple; but hen the ſmall pox came out, theſe ſpots grew gradually leſs, nd at laſt quite diſappear'd. The other child when very oung had been ſubject to convulſions for a long time, and it as very much afflicted therewith from the time that the fever ame on, till the ſmall pox appear'd.

A married gentlewoman, about 26 years of age, got very ell over the diſtemper; but about a week after, ſhe was eis'd with a very great coldneſs and ſhivering, which were ollowed by a burning heat, with a great pain and diſorder in er head, which continued for ſeveral hours. Some time efore ſhe had an intermitting fever, of which he took this to e a paroxyſm, and expected its return; but ſhe felt no more f it, and ever after continued in good health.

The younger of her two ſons, who were both inoculated at he ſame time, got over the diſtemper with a great deal of aſe, the ſmall pox being few, and the ſymptoms very ſlight; ut the elder, a boy about five years of age, far'd quite otherways: The ſymptoms before the eruption were more than ſually ſevere, eſpecially the vomiting; the puſtules appear'd t the uſual time, but more numerous than ordinary, and hen the eruption was finiſh'd, the fever did not ceaſe, as it ad done in every inſtance but this: On the fifth day after he as ſeis'd, the ſwelling of his face began, which was followed

 by

by a pain and ſwelling in his throat and a ſalivation, which continuing till the 11th day, were ſucceded by a ſwelling in his hands and feet, the uſual ſymptoms of the diſtinct ſort, when they are very full; and tho' there appeared ſome little ſigns of malignity; yet with the uſe of bliſters and the milder cordials and alexipharmics, the puſtules roſe very large, and every thing went on very well; ſo that he got over the diſtemper without any danger, but with much more pain and trouble than any of the reſt had endur'd. After the ſmall pox were gone off, there was a hard ſwelling on his ſhoulder, which diſſabled him for ſome time from moving his arm, but by the uſe of ſome common applications, it was entirely diſcuſſed. The only child they had in this family before theſe died of the ſmall pox, of a malignant ſort, and this boy was of an ill habit, and had ſeveral dangerous illneſſes.

One of the thighs of a girl about nine months old after ſhe was well of the ſmall pox, was a little ſwell'd, which was painful to her for ſome time, and made her unwilling to move that part; there were alſo ſome ſmall tumours in the groin; but theſe went off in a few days, there only remaining a hardneſs about the knee, which alſo diſappear'd in a ſhort time, without coming to a ſuppuration, and the child was reſtored to perfect health.

A girl ſix years of age got very eaſily over the diſtemper, but before the ſmall pox were gone, there was a little tumour on the muſcles of the loins, which ripen'd very ſpeedlly, and was open'd and heal'd up in a very ſhort time. Her ſiſter, a young woman about 18, had alſo a ſwelling of the ſame kind in her leg, but it lay ſomewhat deeper, and gave her a great deal of pain for three or four days; afterwards it diſcharged a great quantity of matter, and heal'd without any farther trouble.

All the reſt, excepting thoſe above-mentioned, got over the diſtemper without any manner of trouble or hazard or any ill conſequence afterwards: Whether thoſe ſlight indiſpoſitions which ſome have been ſubject to afterwards, were owing to the inſition, the Dr. could not determine: But he preſumes, that what they have endur'd in the courſe of the diſtemper, and what has followed after, is not to be put in the balance with what is undergone in the common way, by thoſe who are thought to come off very well; and if this method were more generally practis'd, it is probable ſome means might be found out, to prevent even theſe ſubſequent diſorders

orders, which are no more frequent, nor near ſo bad, as thoſe which follow the natural ſort.

In two inſtances the inoculation had no effect; the reaſon of which in one was, becauſe the child had had the ſmall pox before, as the parents believ'd; but the diſtemper had been ſo favourable, as to leave it doubtful. In the other the matter was taken, when the puſtules were wither'd and almoſt gone, and that little moiſture which they contain'd, the Dr. ſuppoſes had loſt its virtue; the boy, on whom it was made uſe of, was no ways affected; the places of inciſion did not at all inflame or ſwell, as uſual, nor did any puſtules appear; but about a month after, he was ſiez'd with the diſtemper in the ordinary way, and did very well.

Some of thoſe who had been inoculated, and grown up, have afterwards attended others in the ſmall pox; and it has often happened, that in familes where ſome children have been inoculated, others have been afterwards ſeis'd in the natural way, and they have lain together in the ſame bed all the time; but it has not hitherto been found, that ever any had the ſame diſtemper twice; neither is there any reaſon to ſuppoſe it poſſible, there being no obſervable difference between the natural and artificial ſort; but only that in the latter the puſtules are commonly fewer in number, and all the reſt of the ſymptoms in the ſame proportion more favourable: There is one obſervation the Dr. has made, tho' he would not lay any great ſtreſs upon it; and that is, that in families where any have been inoculated, thoſe who have been afterwards ſeis'd, never had an ill ſort of ſmall-pox, but always recover'd very well.

A farther Account of inoculating the Small-pox; *by the Same.* Phil. Tranſ. N° 370. p, 49.

THE hazard in this method is very inconſiderable, and in proportion to that in the ordinary way by accidental contagion, ſo ſmall, that it ought not to deter any body from making uſe of it.

The Dr. in order to ſatisfy himſelf what proportion the number of thoſe that die of the ſmall-pox might bear to the whole number, ſeis'd with the diſtemper in the natural way, made ſome enquiry, as follows.

In *Halifax*, ſince the beginning of the winter 1721, 276 had the ſmall-pox, out of which 43 died. In *Rochdale* a ſmall neighbouring market town, 177 had the diſtemper, of which

38 died. In *Leeds* 792 had the ſmall-pox, of which 189 died. It is to be noted, that at *Halifax* the ſmall-pox were this ſeaſon more favourable than uſual; and in *Leeds* they were more mortal than uſual: But at a medium in theſe three towns, there died 22 nearly out of every hundred, which is above $\frac{1}{5}$ part of all that were infected in the natural way.

In theſe accounts the Dr. confines himſelf to the limits of the towns; the numbers, that had the ſmall-pox in the country round about, were vaſtly greater; but the proportion of ſuch as died is much the ſame. He made enquiry in ſeveral country villages about *Halifax*; in ſome he found the proportion to be greater, in others leſs, but in the main it was nearly the ſame.

Obſervations of the Variation on board the Royal African Pacquet, *in the Year* 1721; *by Capt.* Cornwall. Phil. Tranſ. N° 371. p. 55.

N. B. The meridional diſtance is reckoned from St. *Jago.*

Month and Year.	Latitudes.	Meridional Diſtance.	Longitud.	Variation.
Auguſt 24*th* 1721	9° 8′ Sou.	9° 23′ W	9° 25′ W	2° 13′ E
Ditto 26	11 12 S	10 46 W	10 50 W	4 30 E
Ditto 27	11 34 S	11 28 W	11 41 W	4 29 E
Ditto 28	12 32 S	11 31 W	11 43 W	4 27 E
Ditto 31	15 46 S	10 53 W	11 6 W	6 10 E
Septemb. 2*d.*	16 26 S	8 25 W	8 30 W	7 16 E
Ditto 5th	18 45 S	9 31 W	9 39 W	6 17 E
Ditto 6th	19 47 S	9 10 W	10 0 W	8 6 E
Ditto 17	28 43 S	1 7 W	1 9 E	5 53 E
Ditto 22	31 33 S	3 41 E	3 56 E	4 10 E
Ditto 27	33 30 S	11 29 E	12 57 E	0 11 W
Ditto 30	32 40 S	19 6 E	12 1 E	3 0 W
October 1*ſt*,	32 53 S	21 18 E	24 59 E	5 41 W
Ditto 3	32 30 S	25 33 E	30 0 E	7 47 W
Ditto 5	32 28 S	30 37 E	35 52 E	8 44 W
Ditto 6	31 22 S	31 40 E	37 7 E	10 57 W
Ditto 7	31 11 S	32 4 E	37 47 E	11 20 W

Obſer-

Observations on the coast of *Africa*.

Month and Year	Latitudes.	Meridional Distance.	Longitud.	Variation.
Octob. 13*th*, 1721.	26° 17′ S	35° 35′ E	41° 41′ E	14° 30′ W
Ditto 19	19 41 S			12 22 W
Ditto 21	17 4 S			14 29 W
Ditto 25	13 56 S			14 48 W
Novem. 4*th*,	10 57 S			13 11 W
Ditto 7	8 19 S			15 14 W
Ditto 29	5 0 S	in *Cabenda-Bay*		14 33 W
From *Cabenda* to *London*, Meridional Distance from thence				
Decem. 9*th*,	3 25 S	11 38 W	11 43 W	11 32 W
Ditto 14	3 30 S	21 18 W	21 24 W	
Ditto 20	0 30 S	30 41 W	30 46 W	1 5 W
Jan. 1*st* 172½	10 50 N	39 8 W	39 16 W	1 1 E
Ditto 6	17 15 N	43 21 W	43 29 W	1 41 E

Remarks on an Experiment, whereby it has been attempted to shew the falsity of the common Opinion, in relation to the Force of Bodies in Motion; by Dr. Pemberton. Phil. Transf. N° 371. p. 57.

DR. *Pemberton*, perusing the learned *Polenus*'s tract *de Castellis*, found several curious experiments therein; among which he reckons that of letting globes of equal magnitude, but of different weights, fall upon a yielding substance, as tallow, wax, clay, &c. from heights reciprocally proportional to the weights of the globes. This experiment, being brought on purpose to overturn one of the first principles, established in natural philosophy, the Dr. can by no means admit of the deduction drawn from it, *viz.* that because the globes make in this experiment equal impressions in the yielding substance, therefore, they strike upon it with equal force; whereby it is attempted to prove the assertion of M. *Leibnitz*, that the force of the same body in descending is proportional to the height from whence it falls; or in all motion, proportional to the square of the velocity, and not proportional to the velocity itself, as is commonly taught. On the contrary, the Dr. thinks, that this very experiment proves the great unreasonableness of M. *Leibnitz*'s notion.

It

It is ſurpriſing, that ſo careful a writer, as *Polenus* appears to be, from the accuracy with which he delivers his experiments, ſhould not rather ſuſpect his reaſoning in an intricate caſe, than thus contradict a principle in philoſophy, that has been directly proved by a multitude of experiments, in particular, by thoſe Sir *Iſaac Newton* recommends for that purpoſe *Princip. Philoſ. Nat. p.* 19. and that is, moreover, abundantly eſtabliſhed, by its exact agreement with all obſervations; as being the principle upon which all appearances, hitherto obſerved in the motion of bodies, are accounted for by juſt and undeniable arguments; and we ſhall find upon enquiry, that the preſent caſe comes alſo under the ſame rule.

As the uſe of experiments in *Natural Philoſophy* is to diſcover the cauſes of things, by exhibiting more ſimple effects of thoſe cauſes, than occur in the ordinary courſe of nature: So for this end it is neceſſary, that our argumentation upon experiments be perfectly juſt, otherways they may lead us into errors. The firſt thing, neceſſary for making right deductions from an experiment, is to determine the proper uſe thereof; which, in the experiment before us, the Dr. thinks is not rightly underſtood. Certainly this experiment of *Polenus* is much more adapted to inform us of the law, by which theſe yielding ſubſtances reſiſt the motion of bodies ſtriking upon them, than to ſhew the forces with which bodies ſtrike; for, whatever thoſe forces be, the effects muſt be very different, according to the difference there may be in the rule, obſerved by ſuch reſiſtance.

Now this experiment ſhews, that if two globes in motion bear againſt equal portions of the yielding ſubſtance, the oppoſition, that ſubſtance makes to the motion of the globes, will be the ſame in both, however different the velocities be, with which they move. This the Dr. demonſtrates as follows.

Let A and B be two globes of equal magnitude but of different weights, which are equally immerſed into a yielding ſubſtance: Suppoſe the velocities with which they move in their preſent ſituation, to be reciprocally in the ſubduplicate ratio of the weights of the globes; that is, let the ratio of the weight of the globe A to the weight of the globe B, be duplicate of the ratio of the velocity of the globe B, to the velocity of the globe A: Since therefore the ratio of the quantity of motion in the globe A, or of the force with which it moves, to the quantity of motion in the globe B, or to the force with which

which that globe moves, is compounded of the ratio of the weight of the globe A to the weight of the globe B and of the ratio of the velocity of the globe A to the velocity of the other globe B; the force, with which the globe A moves, is to the force with which the globe B moves, as the velocity of this globe B to the velocity of the other globe A. But if the same opposition be made to the motion of the globes, when they bear upon equal portions of the yielding substance, the effect of that opposition, while the globes enter farther into the substance by equal spaces, will be proportional to the time, in which the globes are moving those spaces, or in which the opposition is made, if we consider those spaces while nascent, or in their first origin; the effect therefore of this opposition will be reciprocally proportional to the velocity of each globe; namely, the momentaneous loss of force in the globe A will be to the momentaneous loss of force in the globe B, as the velocity of the globe B to the velocity of the globe A; and the whole force of the globe A has been found to bear the same ratio to the whole force of the globe B; consequently these globes, while they penetrate equal spaces into the substance, lose parts of their force, that bear the same proportion to the whole; and therefore, if their velocities be at any time reciprocally in the subduplicate ratio of their weights; so that the forces or degrees of motion, with which they move, be reciprocally proportional to their velocities, the forces with which they press, into the yielding substance, at equal indentures, made in the substance, will continue in the same proportion; and therefore, upon the theory of resistance here supposed, when the whole force and motion of both these globes is entirely lost, they will be plunged into the substance, at equal depths.

Now whereas in the experiment of *Polenus*, the globes falling from heights reciprocally proportional to their weights, strike upon the yielding substance with velocities reciprocally in the subduplicate ratio of their weights, and the effect is in all cases found to be what is here deduced from this theory of resistance, it is a sufficient confirmation of the truth of this theory.

Only here the Dr. supposes the globes to be stopped by the whole resistance of the substance they move against; tho' strictly speaking, they are stopped only by the excess of that resistance above the action of gravity upon them: But he neglects the consideration of the action of gravity, that being but

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It is ſurpriſing, that ſo careful a writer, as *Polenus* appears to be, from the accuracy with which he delivers his experiments, ſhould not rather ſuſpect his reaſoning in an intricate caſe, than thus contradict a principle in philoſophy, that has been directly proved by a multitude of experiments, in particular, by thoſe Sir *Iſaac Newton* recommends for that purpoſe *Princip. Philoſ. Nat. p.* 19. and that is, moreover, abundantly eſtabliſhed, by its exact agreement with all obſervations; as being the principle upon which all appearances, hitherto obſerved in the motion of bodies, are accounted for by juſt and undeniable arguments; and we ſhall find upon enquiry, that the preſent caſe comes alſo under the ſame rule.

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ſmall in proportion to the reſiſtance, as will appear from the globes being much more ſpeedily ſtopped by this reſiſtance, than they would be by the action of gravity, if its force were applied upwards; for, by that force alone, the globes would not be ſtopped, till they had gone over ſpaces, equal to the heights above the reſiſting ſubſtance, from whence they fell; which heights bear a great proportion to the depths into which the globes in this experiment are immerſed in the yielding ſubſtance, as he found upon trial.

Thus, the Dr. takes it, the difficulties attending this experiment may be removed: But as M. *Leibnitz*'s opinion is deduced from it by means of this axiom, that effects are proportional to their cauſes; ſo that here the effects being thought the ſame, the cauſes are concluded to be ſo likewiſe; the Dr. mentions an experiment, where this axiom may be more juſtly applied, than it can be in the preſent caſe, from which experiment the received opinion may be proved: This experiment is mentioned by *Polenus* from *Merſennus*, tho' tried by him ſomewhat rudely; but has been often ſince made in the following manner; a weight is ſuſpended at one end of the beam of a balance, and from a proper height is let fall upon the other end of the beam another weight, which by ſtriking thereon ſhall raiſe the end, to which the weight is ſuſpended, to ſuch a height, as is juſt ſufficient to ſet free a certain ſpring; if then a different weight be hung in the room of the former, the height from which the falling weight muſt deſcend, in order to raiſe the extremity of the balance, to which this other weight is ſuſpended, to the ſame height as before, that is, high enough to ſet the aforeſaid ſpring at liberty, is found to be ſuch, that the velocity, with which the falling weight ſtrikes upon the balance, in this latter caſe, will be to its former velocity, as the latter weight to the former, vide *Graveſande's Phyſic. Elem. Tom.* I. p. 39. excepting only, that agreeably to what *Merſennus* himſelf remarked, when the weight deſcends from conſiderable heights, an altitude, ſomewhat greater than this rule implies, is required to raiſe the other weight, as much as is deſired: But whether the bending of the arm of the balance, when acted upon with a conſiderable force, or whether any increaſe of friction in this caſe, occaſion the irregularity here mentioned, we need not ſtrictly enquire; for, this irregularity is ſtill leſs reconcileable with the new opinion than the regular effects of the experiment. Hence, therefore, we may ſee, that the very method of reaſoning, which being applied erro-

erroneously, is supposed to prove M. *Leibnitz*'s sentiment concerning the force of bodies in motion, will, when justly used, confirm the other opinion in relation to that matter.

That the very experiment of *Polenus* is not only reconcileable to the common doctrine of motion, as the Dr. has now demonstrated; but even that it does itself make manifest the great unreasonableness, if not the absolute absurdity of M. *Leibnitz*'s opinion, he proves, as follows.

If two globes as A and B, of equal magnitude, but different weights, striking on a yielding substance, with equal force, in every case lose all their motion at equal depths; it is necessary, that at all times, during their motion, they lose equal degrees of force, when they bear upon equal portions of the substance, in entering equal spaces thereof. This will be easily seen from what has been said before. Now, whereas M. *Leibnitz* supposes the power of gravity to give to the same falling body degrees of force, proportional to the height from whence it falls, according to his opinion, by the power of gravity, equal degrees of force are added in the descent of the same body thro' equal spaces; and in different bodies, descending thro' equal spaces, the degrees of force added will be as the quantity of matter, or as the weight of each body. Therefore while the globes A and B penetrate equal nascent spaces into the yielding substance, by the action of gravity, were not that action overcome by the resistance of that substance, additional degrees of force would be communicated in such proportion, that the force added to the globe A, would be to the force added to the globe B, as the weight of the globe A, to the weight of the globe B, or in the duplicate ratio of the velocity of the globe B, to the velocity of the globe A. But since the globes lose the same degrees of force in entering equal nascent spaces into the yielding substance, the effect of the opposition made by this substance to the motion of the globes, during the time of their passing thro' such nascent spaces, will be both the taking from them that same degree of force, and moreover the additional force, which would otherways have been given them by their own gravity: But farther, the opposition made to the motion of the globe A to the opposition made to the motion of the globe B, will be in the ratio compounded of the ratio of the effect of the opposition, the substance makes to the motion of the globe A, to the effect of the opposition, the substance makes to the motion of the globe B, and of the ratio of the time, in which the opposition is made against the latter globe, to the time in which it is made against the former; which latter ratio is

is the ſame with the ratio of the velocity of the globe A to the velocity of the globe B. But ſince it is ſhewn, that the effect of the oppoſition made by the yielding ſubſtance to theſe globes is twofold; and that one part of the effect of the oppoſition made to the motion of the globe A is equal to one part of the effect of the oppoſition, made to the motion of the globe B; and that another part of the effect of the oppoſition made to the motion of the globe A, to another part of the effect of the oppoſition made to the motion of the globe B, is in the duplicate ratio of the velocity of the globe B, to the velocity of the globe A; one part of the oppoſition itſelf made to the motion of the globe A will be to one part of the oppoſition againſt the motion of the globe B, as the velocity of the globe A, to the velocity of the globe B; and another part of the oppoſition to the motion of the globe A, to another part of the oppoſition to the motion of the globe B, will be as the velocity of the globe B, to the velocity of the globe A: So that when the globes bear upon equal portions of the yielding ſubſtance, the oppoſition to their motion will be in part as the velocity of the globes; and in part reciprocally as their velocity. Hence becauſe the reſiſting ſubſtance is of an uniform texture, the oppoſition to the motion of either of the globes in its preſent ſituation, and when moving with its preſent velocity, will be to the oppoſition it would meet with in the ſame ſituation, if it moved with any other velocity, partly as the preſent velocity to that other velocity, and partly as that other velocity to the preſent. But by that part of the oppoſition made againſt the motion of the globe, which is directly as the velocity, the globe can never be wholly ſtopped; for, upon the ſtopping of the globe, that part of the oppoſition to its motion will likewiſe totally ceaſe; and conſequently, the weight of the globe will carry it farther down, unleſs the other part of the oppoſition againſt its motion prevent it: But, neither can this latter part of the oppoſition made to its motion be ever great enough to ſtop the globe; for, the degree of this oppoſition being reciprocally as the velocity of the globe, when the motion of the globe is entirely taken away, it will become infinitely greater than at any time, while the globe is in motion; ſo that when the globe ſhould be ſtopped by this part of the oppoſition made to its motion, the oppoſition to the motion of the globe will become infinitely great; inſomuch, that no degree of force whatever could be able to impel the globe farther into the ſubſtance; but this can never happen: Beſides, it is not neceſſary to apply any ſuch refined argument againſt this part of the

he reſiſtance; it would be alone ſufficient to conſider, how un-aſonable a ſuppoſition it is, that a reſiſtance ſhould increaſe, hen the velocity of the reſiſted body decreaſes.

Thus may this experiment be made uſe of to invalidate that ery opinion it is brought to ſupport: But another uſe may kewiſe be made thereof; for it will ſerve to illuſtrate what he great Sir *Iſaac Newton* has more than once hinted, *viz.* hat the reſiſtance of fluids, ariſing from the tenacity of their arts, decreaſes in a leſs proportion than the velocity of the eſiſted body does. vide *Philoſ. Nat. Prin. Math. Prop.* 2. *lib.* 2. in *Schol. Optics Qu.* 28. *p.* 339, 340. for, as this eſiſtance bears a great analogy to the reſiſtance of the yield-ng ſubſtances here treated of; ſo it is found, that the reſiſ-ance of theſe ſubſtances does not much depend upon the velo-ity of the body, againſt which the reſiſtance is applied.

And thus we may ſee, how all experience conſpires in con-rming and ſetting forth that ſtupendious force of reaſoning, hich has enabled our great philoſopher moſt ſurpriſingly to earch out and diſtinguiſh the ſprings of natural operations; work infinitely more difficult to accompliſh than even the reat improvements he has made in pure mathematics, which ere previouſly neceſſary in order to his ſucceeding in his re-earches into the knowledge of nature; for, in this laſt purſuit e has given proof, not only of a more unbounded invention, han is required in the ſubtileſt geometrical ſpeculations; but as alſo there diſcover'd the greateſt diſcernment and moſt onſummate judgment: Since in his philoſophical writings, he as never been once impos'd on by an hypotheſis, nor by any ther of the various fallacies, which my Lord *Bacon* in his *Novum Organon* has reckoned up as the cauſes, that had hin-der'd the improvement of the true philoſophy.

Another very curious and weighty argument to confirm Sir *Iſaac Newton*'s ſentiments in relation to the reſiſtance of fluids, s as follows.

Suppoſe pieces of fine ſilk or the like thin ſubſtances, ex-tended in parallel planes and fixt at ſmall diſtances from each other; then ſuppoſe a globe to ſtrike perpendicularly againſt the middle of the outermoſt of the ſilks, and by breaking thro' them to loſe part of its motion: If the pieces of ſilk be of equal ſtrength, the ſame degree of force will be requiſite to break each of them; but the time in which each piece of ſilk reſiſts, will be ſo much ſhorter, as the globe is ſwifter; and the loſs of motion in the globe, conſequent upon its breaking thro' each ſilk, and ſurmounting the reſiſtance

 thereof

thereof, will be proportional to the time, in which the silk opposes itself to the motion of the globe: Insomuch that the globe by the resistance of any one piece of silk will lose so much less of its motion, as it is swifter. But on the other hand, by how much swifter the globe moves, so many more of the silks it will break thro' in a given time: Whence the number of the silks which oppose themselves to the motion of the globe in a given time, being reciprocally proportional to the effect of each silk upon the globe, the resistance made to the globe by these silks, or the loss of motion the globe undergoes by them in a given time, will be always the same.

Now if the tenacity of the parts of fluids observe the same rule, as the cohesion of the parts of these silks; namely, that a certain degree of force is required to separate and disunite the adhering particles, the resistance arising from the tenacity of fluids must observe the same rule, as the resistance of the silks; and therefore in a given time the loss of motion a body undergoes in a fluid by the tenacity of its parts, will in all degrees of velocity be the same; or in a word, that part of the resistance of fluids, which arise from the cohesion of their parts, will be uniform.

An Account of the Falls of the River Niagara, *taken at* Albany Oct. 10. 1721. *from M.* Borassaw, *a Native of* Canada; *by Mr.* Dudley. Phil. Transf. N° 371. p. 69.

THE falls of *Niagara* are a mighty ledge or precipice of solid rock, that lies a-cross the whole breadth of the river (a little before it empties itself into or forms the lake *Ontario*) and very steep.

M. *Borassaw* says, that in spring, 1722, the Governor of *Canada* order'd his own son with three other officers to survey *Niagara* and take the exact height of the cataract, which they accordingly did with a stone of half a hundred weight and a large cod-line, and found it upon a perpendicular no more than *vingt et six bras*.

This account differs from that F. *Hennepin* has given the world of that cataract; for, he makes it 100 fathoms; and our modern maps from him, (as Mr. *Dudley* supposes) mark it at 600 foot; but he believes *Hennepin* never measured it, and there is no guessing at such things.

Upon Mr. *Dudley*'s objecting *Hennepin*'s account of those falls to M. *Borassaw*, he replied; that accordingly every body had depended upon it as right till that late survey. Upon farther conversation he acknowledged, that below the cataract

for a considerable way, there were a great many small ledges or stairs cross the river, that lower'd it still more and more, till you come to a level: So that if all the descents be put together, he does not know but the difference of the water above the falls, and the level below may come up to F. *Hennepin*'s account; but the cataract, properly so call'd, is, upon the perpendicular, no more than 26 fathoms or 156 foot, which is a prodigious thing, and what the world cannot parallel, considering the greatness of the river; for, it is near ¼ of an *English* mile broad, and very deep water.

It has been alledged, that the cataract makes such a prodigious noise, that people cannot hear each other speak, at some miles distance; but M. *Borassaw* affirms, that you may converse together close by it.

It has been positively asserted, that the shoot of the river, when it comes to the precipice, was with such a mighty force, that men and horse might go under the body of the river without being wet; this M. *Borassaw* utterly denies, and says the water falls in a manner right down.

What M. *Borassaw* farther observ'd was, that the mist or shower *(la brume)* which the falls occasion, is so extraordinary, as to be seen at five leagues distance, and rise as high as the common clouds: In this *brume* or cloud, when the sun shines, there is always a glorious rainbow.

The river itself, which is there call'd the *Niagra*, is much narrower at the falls than either above or below; and from below there is no coming nearer the falls by water than about six *English* miles, the torrent is so rapid and withal such terrible whirl-pools.

M. *Borassaw* confirms F. *Hennepin*'s and M. *Kellug*'s account of the large trouts of those lakes, and he affirm'd there was one taken, that weigh'd 86 pounds, which Mr. *Dudley* is the more apt to believe upon the general rule, *viz.* that fish are according to the waters: To confirm which, a minister affirm'd, that he saw a pike taken in *Canada* river and carried upon a pole between two men, that measur'd five foot 10 inches in length, and proportionably large.

Mr. *Dudley* saw a cataract, three leagues above *Albana* in the province of *New York*, upon *Schenectada* river, call'd the *Cohoes*, not above 40 or 50 foot perpendicular: From these falls there also arises a misty cloud, which descends like small rain, and when the sun shines, yields a beautiful small rainbow, that moves as you move, according to the angle of vision. The river at the *Cohoes* is from 40 to 50 rods broad; but then it

it is very shallow water; and he was told, that in a dry season, the whole river runs in a channel of no more than 15 foot wide.

In his journey to *Albany*, 20 miles to the eastward of *Hudson's* river, near the middle of a long rising hill, Mr. *Dudley* met with a brisk noisy brook, sufficient to turn a water-mill, and having observ'd nothing of it at the beginning of the hill, he turn'd about and followed the course of the brook, till at length he found it come to an end, being absorbed and sinking into the ground; either running thro' subterraneous passages, or soak'd up by the sand; and tho' it be common in other parts of the world for brooks and even rivers to be thus lost; yet this is the first of the sort, he ever heard of or met with in this country.

An Account of the Muscular Fibres in several Animals, and the Magnetic Quality acquir'd by Iron, upon standing for a long Time in the same Position; by M. Leewenhoeck. Phil. Transf. N° 371. p. 72.

M. *Leewenhoeck*, upon viewing a portion of the flesh of a fat ox, as likewise the muscular fibres of a cod-fish and of a pearch, that were cut transversely, could very distinctly observe the great number of small vessels, that ran along the length of each fibre; and he has seen the same in the muscular fibres, taken from the hinder leg of a mouse, and cut thro' transversely.

M. *Leewenhoeck* had standing before a microscope some of the muscular fibres of a fat ox, together with those of a mouse lying by them, in order to have as many eye-witnesses as possible of their being of the same size in those two animals; and he used the same method in such other of his observations, as were likely to appear incredible, letting the object lie before the microscope day after day, and sometimes for whole years together, till it was eaten up by insects.

As to the small fibrils, mentioned in a former *Transaction*, that help to suspend the *testes*, it is to be observ'd, that each of them consists of exceeding small vessels, which run parallel to its length.

M. *Leewenhoeck* had likewise before his microscope, a small portion of an ox's bone, in which might plainly be observ'd, the vessels which proceed from the bone and compose what is call'd the *periosteum*, as likewise the openings of these vessels; the reason of whose appearing so plainly is, as he supposes, because they are fill'd with the medullary oil.

The

The iron croſs, which is ſuppos'd to have ſtood upon the ſteeple of the new church at *Delft* about 200 years, having been taken down to be repair'd, and M. *Leewenhoeck* being inform'd by a certain foreign gentleman, that a piece of iron, that has ſtood for a long time in one poſition, would thereby acquire a magnetic quality, he procur'd a bit of that croſs, of about a ſpan long and a quarter of an inch thick, which he applied both to a working needle and the needle of the compaſs, but without any effect upon either.

Some time after, M. *Leewenhoeck* had brought him ſome other pieces, looking like ruſty iron, that had been broke off from the bottom of the ſaid croſs, where it had been faſtened by four croſs pieces, bound down with iron, to an erect piece of timber nine inches ſquare, and cover'd with lead in ſuch a manner as no wet could get to it.

This ſeeming ruſty iron would take up ſeveral needles hanging by each other; and it appear'd to have a ſtronger magnetic virtue than two loadſtones, he had then in his cuſtody, and ſo hard that no file would touch it.

M. *Leewenhoeck* gave one of the largeſt pieces to a knife-grinder, to grind, who was a long time about it, and complain'd that it was harder than ſteel.

The Method of bending Planks *in his Majeſty's Yards at* Deptford, *&c. by a Sand-heat, invented by Capt.* Cumberland; *by Mr.* Cay. Phil. Tranſ. N° 371. p. 75.

THE place, where the planks lie to be ſoftened in the ſtove, is between two brick walls, of ſuch a length, height and diſtance from each other, as is ſufficient to admit the largeſt or to hold a good number of the ſmaller ſort; the bottom is of thick iron plates, ſupported by ſtrong bars; under the middle of which are two fire-places, whoſe flews carry the flame towards the ends.

The planks are laid in ſand; the loweſt about ſix or eight inches above the iron plates; they are well cover'd with the ſand and boards laid over all to keep in the heat; the ſand is moiſtened with warm water (for which purpoſe they have a cauldron adjoining to the ſtove) and if the timber be large and intended to be very much bent, ſo that it muſt lie long in the ſtove, they water the ſand again, once in eight or ten hours. When it is judged to be ſoft enough, the ſand is remov'd; and the workmen carry away their reſpective planks to the ſeveral places, where they are to be us'd, and having firſt

first nail'd a thin board upon the outside to preserve the plank from bruises, they fix one part in its proper place, and bring to the others, by any power they can most conveniently apply. This work seems to be performed with very great ease; notwithstanding some were so knotty, that the builders assur'd, they could not be brought to that curvature by the former methods. The planks put in between others, very exactly fitted the spaces they had been cut for; and the workmen told, they had made no allowance for the swelling or shrinking of the wood.

This method excells that of burning the planks over an open fire, in several respects; particularly, that no part of the wood is destroy'd, but remains of the same dimensions, at least very nearly; a plank of the breadth of 16 inches being said not to alter above $\frac{1}{20}$ part of an inch; the edges of the plank are preserv'd; and consequently, the work must be much firmer, and the calking last longer; the extraordinary softness of the wood, while it is warm, makes it easily bend to any figure, necessary in ship-building, which it holds very well if they have occasion to take it off again after it is cold: Whereas the plank, bent by burning, would start, when loosened; and could only be fixt to the timbers by such a force, as could overcome the resistance, occasioned by the spring of the plank: It likewise adapts itself very readily to the surface of the timbers, if they happen to be uneven.

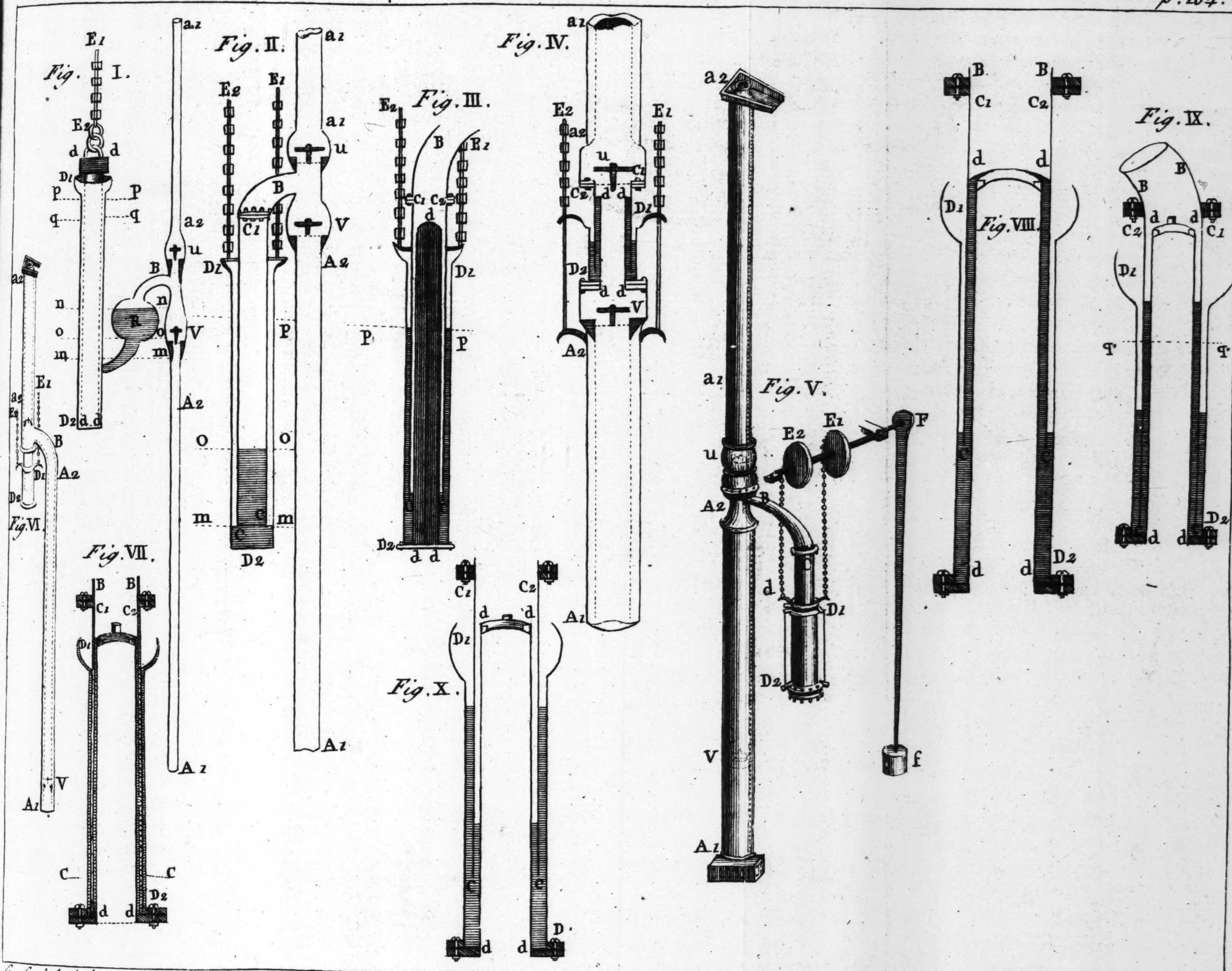
The gun-deck clamps in a ship of the second rate; which are very large planks, and wont to be cut into shape, were bent and twisted in so peculiar a manner, as could not be effected by any other method: The whole operation is perform'd with much less trouble to the carpenters, as well as at less expence; and they hope the wood will be more durable; as is evident from the deep tincture the sand receives, by a considerable quantity of sap coming out of the oak, while it is in the stove; and a large plank was observ'd by the workmen or officers belonging to the yard, to weigh some pounds less, when it was taken out.

A plank five inches thick requires five or six hours to make it fit for bending; and the time requisite for others seems to be in a duplicate ratio of their thickness.

Fig. 1. Plate VI. represents a plank in the buttocks of a second rate, whose length from A to C is three foot, and thickness A F four inches and $\frac{1}{4}$; the end C of this plank was bent 12 or 13 inches from the streight line A B.

Fig.

PLATE. V.



G. Smith del. Hulett Sculp

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Fig. 2 and 3. repreſent ſections of the ſtove; A A the fire-laces; B B the aſh-holes; C C the flews under the iron bottom; DD the two chimneys; E the place for the planks and ſand; F F he two brick-walls; G G two inclined planes for the men to tand on, &c. when they put in or take out planks, or water the and; *b b* the bottom of the ſtove, made of iron; *i i* the grates o lay the fewel on.

An Account of two Caſes of Wounds *in the* Stomach; *by Mr.* James Field. Phil. Tranſ. N° 371. p. 78.

A Luſty young *Negro* in *Antegoa*, returning home about noon, went into his houſe, where ſeeing ſome ripe plantains, e eat of them heartily; his father-in-law, about 60 years of age, oming to the ſame houſe ſoon after, and finding the young fellow ad taken his plantains, gave him a moſt deſperate wound with is knife in the upper region of the belly, a vaſt gaſh being ade in the ſtomach; inſomuch that the plantains he had eaten, urſt thro' the wound, which was made ſtreight up and down.

The old man immediately fled for it; the young fellow's companions upon hearing what was done purſued him with bills in eir hands: The old man ſeeing them follow him in this manner nd get ground of him, and ſuſpecting their deſign was to kill im, pulled out the ſame *knife*, with which he had ſtabb'd the ther, and gave himſelf as deſperate a wound, as he had given im, and in the upper region of the belly, his ſtomach being kewiſe ſeen, only with this difference, that this laſt wound was anſverſe or from left to right, the firſt directly up and down: he old man was carried home and laid in the ſame houſe, here the other lay.

This accident happened about noon, and the ſurgeon did not me to dreſs them till between four and five; he ſtitched up both eir ſtomachs and bellies, only leaving in each a ſmall hole for ppuration: A fever ſeiſed each of the patients; the old man as in greateſt danger; the fever held them about a fortnight; e wounds were brought to a good digeſtion, and in a month's me or thereabouts the young fellow went abroad; but the old an lay ſomewhat longer: They were both perfectly cured, and ery well ever after.

An Account of an Imposthumation *in the* Stomach; *by Mr.* Atkinſon. Phil. Tranſ. N° 371. p. 80.

MR. *Atkinſon* had a patient, who had a large tumour on the upper part of her belly; it was hard and painful; but did not alter the natural colour of the ſkin and was three months in coming: He applied a warm gum-plaiſter, which in about a fortnight's time brought it to a ſuppuration; he then applied a cauſtic about the bigneſs of a ſhilling; when the eſcar fell off, he obſerved a ſolid kind of ſubſtance appear in the orifice; he laid hold of it with his *forceps* and pulled it gently towards him; upon which there thruſt forcibly out a quantity of it that near filled his hand, and ſo he dreſſed it. Next dreſſing the ſame ſubſtance appeared again, which on her ſtraining, forced out twice as much as before: Mr. *Atkinſon* concluded it was the *omentum*, in which opinion he was confirmed by ſome other ſurgeons, to whom he ſhewed it: He ſtill doubted, whether the ſtomach was concerned in this caſe or not, till the next removal of the dreſſings; at which there ſpurted out upwards of $\frac{1}{2}$ a pint of ale in a full ſtream, which was part of a pint the patient had drank a little before: Mr. *Atkinſon* now concluded the caſe mortal; however he ordered the patient to keep her bed, to lie conſtantly upon her back and feed on things of eaſy digeſtion. The greateſt part of what ſhe eat or drank came thro' the ulcer for 8 or 10 days; ſo that he had no hopes of ever curing it, yet contrary to his expectation in about ſix weeks ſhe was perfectly cured.

An Account of the Quantity of Reſin *in the* Cortex Eleutheriæ; *by Mr.* John Brown. Phil. Tranſ. N° 371. p. 81.

DR. *Douglaſs* having given an account (from the hiſtory of the *Royal Academy* at *Paris*) of the *cortex Eleutheriæ*; and among other things having ſaid of it, that M. *Boulduc* had, by means of ſpirits of wine, gotten from an ounce of the bark five drachms of reſinous extract, there remaining three drachms of *fæces*, and that Gentleman's account of ſome of the properties of this bark being founded on the quantity of reſin, ſuppoſed to be contained therein; Mr. *Brown* propoſed to Dr. *Douglas* and ſome other Gentlemen of the *Royal Society* (who agreed with him in believing, that ſcarcely any part of any plant whatſoever would yield that quantity of reſinous extract) to try the experiment, which was performed in the following manner.

He

He took two ounces of pick'd bark and digested it in rectified spirits of wine, which was often decanted and fresh spirits put on, till the bark would yield no more tincture; the impregnated spirits being evaporated by a very gentle fire; there was left two drachms of resinous extract; the remains of this dried, weigh'd one ounce, two drachms and ½; the loss this way is three drachms and ½.

He boiled these remains in several waters, till they would no longer tinge the water, which being evaporated, yielded a drachm and a half of extract; the remains of this dried, weighed one ounce, and half a drachm; the loss by this method is half a drachm.

He took two ounces more of pick'd bark and boiled it in several waters, till the bark gave no more colour; and then, upon an evaporation of the water, he had two drachms of extract; the remains being dried weigh'd one ounce and six drachms; here the loss was nothing, excepting so much as might answer in weight to the quantity of the menstruum left in the extract, which allowance must likewise be made in the other extracts.

He digested the remains in rectified spirits of wine, till they no longer tinged the spirits; and by a very gentle evaporation, he found remaining one drachm of resinous extract: What was left, when dried, weighed one ounce, two drachms, and a half; in this the loss was two drachms and a half.

The difference in the quantity of extract, obtained by these two different methods, is but half a drachm; and the medium between them, upon putting together the several extracts, made with spirits of wine and water, is in the whole but three drachms and ¼. But the extract made with spirits of wine alone is no more than two drachms from two ounces of the *cortex*, instead of 10 drachms, which it should have yielded according to M. *Boulduc*.

An Account of the new Method of Cutting for the Stone; *by Mr.* Douglas. Phil. Trans. N° 371. p. 83.

EVER since Mr. *Douglas* could reason upon the causes of the tediousness and bad success of the common methods of cutting for the stone, he concluded they were principally owing to the natural structure and situation of the parts concerned.

Therefore, he began to consider, why that operation might not be performed the highway, so frequently mentioned by authors, but never approved of by any, excepting the most sagacious of all surgeons, *Fr. Rossetus*, who, he thinks, has either been very

little read, or very ill underſtood, elſe this operation had not been ſo long a ſecret.

After making ſome experiments on dead bodies, Mr. *Douglas* was convinced the ſtone might be extracted that way with a great deal leſs trouble than in the common way; and he was perſuaded the wound would heal again, by the great number of authentic inſtances of accidental wounds in the ſame place being ſpeedily and firmly cured; and therefore he reſolved to make the experiment on the firſt patient he could meet with, which was in *December* 1719, and then he proceeded in the following manner.

The patient was placed flat on his back on a table, with a pillow under his head; then his wriſts and ankles were faſtened together, with ſtraps; then he ordered one aſſiſtant to his head, another to each of his ſhoulders, two to the *penis*, one of which was to manage the ligature and the other the *præputium*, and one to each knee, to hold them as faſt and firm as poſſible.

The patient and aſſiſtants being thus placed, the operation conſiſts of three parts.

1. In filling the bladder, which is done thus; paſs the catheter, as repreſented by Fig. 4. Plate VI. then draw out the ſtillet Fig. 5. then order the ligature-aſſiſtant to caſt the ligature, which is a ſkein of ſilk, round the *penis*, above the glans; then fix the key Fig. 6. to the head of the catheter Fig. 7, to keep it ſteady, while you ſcrew on the ſyringe Fig. 8. then ſcrew the ſecond part of the ſucking pipe Fig. 9. to the firſt part Fig. 10. then order the *penis*-ligature to be ſtraitened and the *præputium*-aſſiſtant to gather the *præputium* up about the catheter and to hold it as cloſe as poſſible; then order the water, being a little warmer than milk, to be clapp'd under the ſucking pipe; then draw up the water into the ſyringe and thruſt it into the bladder at leiſure, and repeat it till the bladder be ſo full, that you can perceive its tumour thro' the *abdomen* (at which time you may alſo obſerve the *penis* above and the *præputium* below the ligature, very much ſwelled and the patient in a great deal of pain, which is a certain ſign that there is enough injected; then withdraw your ſyringe and catheter together, taking particular care that your *penis*-aſſiſtants ſtraiten their gripe, leaſt the water ſhould follow the catheter, which would undo all.

2. In making the inciſion, which is done thus; order the *penis*-aſſiſtants to turn the *penis* towards the *anus*, that ſo their hands may be the more out of the way, then take the firſt knife, repreſented by Fig. 11. and cut at leiſure and with a ſteady hand from near the upper or lower part of the tumour of the bladder

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according to the computed bigneſs of the ſtone, down to the *os pubis* and exactly in the middle: When you are got a little more than half way thro' the abdominal muſcles, take the ſecond knife, as repreſented by Fig. 12. clap its back on the middle of the *os pubis*, then run its point down towards the *ſphincter*, till you get into the cavity of the bladder, which is diſcovered by the iſſuing out of the water; then run your knife along very quickly towards the fund of the bladder, as far as is neceſſary.

3. In extracting the ſtone, which is done thus; before you withdraw your knife, introduce the fore and middle fingers of your left hand, between the knife and the *os pubis*, into the bladder; then withdraw your knife and thruſt the fore and middle fingers of your right hand into the *anus*, and raiſe the ſtone up towards the wound, and ſo you will eaſily catch hold of it (tho' never ſo ſmall) with your fingers which are in the bladder, then extract it with the ſmalleſt end foremoſt; then introduce your fingers again, to ſee if there be any more ſtones, which are to be extracted as before.

Then take a needle and thread and make one ſtitch thro' the ſkin, in the middle of the wound and tie it pretty cloſe; then undo the ſtraps and carry the patient to bed.

The patient being put to bed, Mr. *Douglas* laid a pledget, armed with balſam over the wound and a bit of ſticking plaiſter over that; then he embrocated all the *abdomen*, *ſcrotum* and *penis*, with warm *Ol. Chamæmel.* then he applied over the dreſſing and all the *abdomen* an emollient poultice, ſpread almoſt an inch thick on ſoft flannel; then he turned a ſwath, a little broader than the patient's hand, once round him and pinned it on the poultice cloth, juſt tight enough to keep it on.

As ſoon as the patient was dreſſed, Mr. *Douglas* gave him an opiate (for nothing is ſo proper as reſt) ſuch as this ℞ *Aq. Cinnam. Hord.* ℥ii. *Laud. liq. gut* xv. *Syr. de Mecon.* ℥ii which may be increaſed or diminiſhed, as the caſe requires.

Next evening Mr. *Douglas* took off the poultice and dreſſing and cut the ſtitch; then fomented the wound and all the *abdomen* with ſtupes, wrung out of *Aq. calc.* and freſh urine, as warm as he could bear it; then dreſſed the wound as before; then he rubbed all the *ſcrotum*, *penis* and groins with *unguentum album*, to prevent their being ſcalded by the urine, which flows from the wound.

The wound muſt be dreſſed twice a day at leaſt, till you have a plentiful digeſtion.

After every dreſſing the ointment and oil was uſed, as before directed.

There is little variation in the dreſſing of the wound, excepting what is common in others.

The urine flows always thro' the wound, till the wound of the bladder be cured, which is ſooner or later according to the conſtitution of the patient.

When the urine begins to come the right way, it pains and ſcalds the patients much after the ſame manner, as when they had the ſtone (which is occaſioned by the contraction of the *urethra* that has been ſo long uſeleſs) but it never laſts above a day or two and then they make water with the ſame eaſe and freedom, as any other perſon.

The patients ſhould not be forced to go to ſtool under ſix or ſeven days, unleſs there is ſome particular reaſon for it; becauſe ſtraining to go to ſtool injures the wound.

They ſhould never be taken up, excepting to get their beds made, till the urine come all the right way, becauſe it makes them ſick and faint; and conſequently hinders the cure of the wound.

Cold is to be avoided as the peſt, becauſe it puts the patient to a great deal of pain either to ſtifle it or to cough out.

N. B. If a flexible catheter could be paſſed and kept in the paſſage without pain, it would very much haſten the cure of the wound.

Mr. *Douglas* performed the operation for the firſt time *December* 23. 1719 upon a boy between 16 and 17 years of age, and in five week time he was perfectly cured.

The ſtone was of the figure and bigneſs, as repreſented Fig. 13.

He made the ſecond operation on the 12. of *May* 1720, on a boy of eight years of age, and in ſix weeks time he was perfectly cured.

The ſtone was of the figure and bigneſs, as repreſented by Fig. 14.

The third patient was but three years of age and was cut in *Auguſt* 1720, but died of convulſions about 15 hours after the operation.

Fig. 15. repreſents the form and bigneſs of the ſtone.

The fourth operation was made *March* 23. 1720-21, upon a boy about 14 years of age, and in four weeks afterwards he was perfectly cured.

Fig. 16. repreſents this ſtone.

In this patient Mr. *Douglas* made a ſmall wound in the *perito-
eum*, thro' which he obſerved the guts to preſent, but he puſhed
em back with his finger and then ſtitched the ſkin, and there
as no farther inconveniency by it.

This high operation may be performed with equal ſucceſs on
males, when the ſtone is large; but if it be but ſmall, the
mmon way of extracting is very good.

From all which Mr. *Douglas* concludes in the words of the
imitable *Roſſetus*.

*Poſt hæc nemini dubium eſſe debet, novam hanc noſtram cyſto-
miam, vetere illâ tot doctiſſimorum Chirurgorum cyſtotomiâ
am periculoſâ, ut eam aggredi vel ipſe Hippocrates Chirurgôn
hirurgotatos metuerit) & leniorem & tutiorem haberi.*

n Account of a Parhelion *obſerved in* Ireland; *by Mr.* Dobbs. Phil. Tranſ. N° 372. p. 89.

MARCH 22. 1721-22, about $\frac{1}{2}$ an hour after five in the af-
ternoon nearly; Mr. *Dobbs*, being in the fields about *Caſtle
obbs* in the county of *Antrim*, obſerved a diſtinguiſhable par-
elion, the ſun being near W. about an hour high, the wind and
otion of the clouds about N. and by E. the ſky obſcured in ſe-
ral places with light clouds, and the ſun entering into one ſome-
hat more watery, yet ſo as to diſtinguiſh its diſk: At firſt ap-
eared below the ſun, breaking out of the cloud, ſuch rays as
e uſually obſerved in an evening, in a ſky interſperſed with
louds. In a little time appeared at the ſame height with the ſun,
s near as Mr. *Dobbs* could conjecture (having no inſtrument) a
minous ſpot, about four times the largeneſs of the ſun's diſk,
nd about 30 degrees diſtant from the ſun to the ſouthward,
hich was covered with the lively ſhades of red and yellow on
e ſide next the ſun, and increaſed in ſplendor (ſo as ſcarce to be
ndured by the naked eye) till it exceeded the brightneſs of the
n, which was then under a thin cloud, ſo as eaſily to perceive
is diſk: After this had appeared about three or four minutes,
r. *Dobbs*, perceiving it to be a real parhelion, began to look
bout for the halo they generally appear in; and as he obſerved
me rays like a glory to point upwards from the ſun, he ſaw in
oſe at the ſame diſtance (being as near as he could conjecture
bout 30 degrees perpendicularly above the ſun) the colours of the
alo appearing as in the luminous ſpot; but inſtead of finding it,
s he expected, in a circle ſurrounding the ſun, it was inverted,
et not circular, but forming an obtuſe angle, the point being
towards

towards the ſun. He then looked towards the northward of the ſun, and as the cloud, which was thicker on that ſide, moved ſoutherly, a luminous ſpot began to appear at the ſame diſtance from the ſun as the other, and in the ſame parallel of altitude, which had the ſame colours towards the ſun, and increaſed in brightneſs, but did not come up to that of the other ſpot, yet was as luminous, as the ſun then appeared; this ſpot was very little bigger than the ſun's diſk. As the cloud moved on, till it came to about 60 degrees to the ſouthward of the ſun, and 30 degrees from the ſpot, at an equal height there appeared another ſpot, tinged with the colours of the rain-bow; the whole appearance laſted $\frac{1}{4}$ of an hour. The reaſon of Mr. *Dobbs* not ſeeing the halo's, which generally appear with them, was, that there was a good deal of clear ſky above the ſun, and the cloud too thick below it.

A Fig. 17. Plate. VI. repreſents the place of the ſun, being nearly weſt about 12 or 13 degrees above the horizon, about an hour before ſun-ſet; B the luminous ſpot, being about 30 degrees to the ſouthward of the ſun, as near as Mr. *Dobbs* could compute (having no inſtrument to take the angle) and in the ſame parallel of altitude, the ſpot was not ſo well defined as in the Fig. being more imperceptibly ſhaded off in the cloud, the two ſemicircular lines next the ſun were thoſe tinged with the colours; that neareſt the ſun being of a deep ſcarlet; the inner one a deep yellow, both the colours being ſoftened, as they fell off from the ſun; all the reſt of the ſpot being an intenſe light, ſo as the naked eye could ſcarce bear it; C the other ſpot to the northward, which appeared ſome time after that, marked B, being not quite ſo large, nor the colours ſo intenſe, but diſpoſed in the ſame way, thoſe next the ſun being red, the next yellow and the reſt white; D a ſpot in the cloud, as it moved ſoutherly, till it come to about 60 degrees diſtance from the ſun, which had the colours as in the other ſpots; that next the ſun being red, the next yellow, but much fainter than in the parhelia; E the appearance of two ſegments of circles, at about the ſame diſtance from the ſun, as the parhelia, being perpendicularly above it; the colours being fainter than in the parhelia, but diſpoſed the ſame way; the lower lines next the ſun expreſſing the red and the upper the yellow.

The colours at D and E, as they were not ſo intenſe, neither were they quite ſo broad, as thoſe at B and C, the two colours being added together were about $\frac{1}{4}$ of the diſk at B, and the

colours;

colours in the ſame proportion at C; the diameter of the parhelion at B being about double the apparent diameter of the ſun, as near as Mr. *Dobbs* could compute.

The centers of the ſegments of the halo's, marked E, if not in the parhelia, were very little below them.

Below the ſun and parhelia the cloud was too thick to diſcover any thing thro' it; and above them, till near the ſegments, marked E, the ſky was ſerene and nothing obſcured; but at E, where the rays, which pointed upwards from the ſun, terminated, it appeared hazy and ſo thick as to reflect the colours.

An Account of the Particles of Fat; *by M.* Leewenhoeck. Phil. Tranſ. Nº 372. p. 93.

IN a former *Tranſaction* M. *Leewenhoeck* affirmed, that the matter, called meal or flower in wheat, rye, barley, oats and in all ſorts of beans, is ſhut up, as it were, in little cells, ſeparated from each other by thin membranes, and theſe thinneſt in wheat.

In his enquiries into what is called the *perioſteum* of an ox or ſheep, M. *Leewenhoeck* often broke in pieces the fat particles thereof, and as often viewed them thro' a microſcope; a few of them he likewiſe placed upon a clean glaſs-plate and afterwards held the ſaid glaſs-plate over a coal fire or the flame of a candle ſo long, till they were all melted and reduced to a liquid matter; ſo that not only the fat, which was ſhut up in the ſkin or pellicle of the fat globules, but likewiſe the pellicle itſelf was reduced to a fluid matter; and thereupon bringing it immediately before his eye and viewing it attentively, he perceived, when the melted fat was cold, that different ſubſtances were incloſed in the ſaid fat globule; there appeared a vaſt number of exceeding ſmall coagulated particles, and the reſt of the parts, of which the fat was compoſed, lay in one ſmooth and even ſubſtance; and he has been conſidering, whether there might not be incloſed in ſuch a globule of fat, ſo many little cells and partitions, as we obſerve in a ſmall grain or ſeed; but if this be ſo, it will remain concealed from our eyes.

Having again carefully viewed theſe coagulated globules of fat, ſeveral of which form one ſmall bubble, M. *Leewenhoeck* often imagined, that he obſerved each of the ſaid ſmall particles provided with ſuch a tranſparent dent, as he before affirmed, the meal globules of wheat to be furniſhed withal: From which obſervation he concluded, that ſince we find, the great Creator of the univerſe has formed all his creatures almoſt in the

the same manner; and that there grows no plant whatever, whose seed is never so small, but there is inclosed in it the same sort of particles, as is found in larger seeds; and tho' they differ in magnitude, yet that they are analogous to each other; we may easily imagine, that a fat globule has a bark or pellicle, as well as any seed, as has been often observed, and that they are furnished internally with particles, analogous to those of the seeds, called wheat, oats, &c. and tho' we render fat fluid by the help of fire, yet will the small particles, inclosed in the fat globules, again reassume their former figure, as M. *Leewenhoeck* formerly said.

Nay, he fancied, tho' it did not appear to his sight, that each fat particle is furnished internally with little cells, like the seeds or fruits of plants.

A butcher having killed a sheep of an uncommon size, weighing 140 pounds, exclusive of the fat, which weighed 51 pounds; so that the whole sheep weighed upwards of 190 pounds; M. *Leewenhoeck* procured a piece of the fat, that grew about the kidneys, imagining, that its fat particles would be of a coarser grain than those of ordinary sheep; for, he often observed that the bigger an ox was, the larger were the fat particles, and that there are not any two of the said particles of one and the same figure, being compressed by other particles with which they are surrounded, as M. *Leewenhoeck* frequently observed; some few of which are represented in Fig. 17. Plate VI. between A B C D.

Now when we meet with one of these little bundles of fat particles, as has frequently occurred, in which the fat particles were four times this thickness, M. *Leewenhoeck* imagined, that such fat particles cannot be produced out of one single adipose vessel; but that out of such a vessel several small sprigs issue forth, and out of each of those small sprigs proceed others still smaller; and that out of these particles one large fat particle is formed like a bunch of grapes.

Now he cut off with a razor the fat in several places of a larger piece, as thin as possible; laying the thin slices upon several glass-plates, he put them upon a coal-fire, so as to cause them to melt; upon which immediately viewing them with a magnifying glass, he observed the pellicles or membranous coats of the fat globules, lying among the melted particles; and in the said particles there was nothing to be perceived but a limpid matter, surrounded with small air-bubbles: But when the fat was congealed, but very little of the membranes could be observed

ſerved, becauſe they were covered with the particles of the , with which theſe pellicles or membranes had been be- e filled.

Fig. 18. repreſents between EFGH a few of theſe pelli- s of the fat globules. During the ſaid obſervation he at- ntively fixed his eye upon the fat particles of the ſheep, t had been melted and were again coagulated; and he ld not but judge, that the ſaid particles, which were ex- eding ſmall, were analogous to that internal matter, with ich ſome of the ſmalleſt little ſeeds are furniſhed; and in great many of theſe exceeding ſmall particles, he could in ar weather diſcover ſome tranſparency.

Moreover, he cut ſlices of the fat as thin as poſſible; yea thin, that five or ſix of them did not weigh a grain, and put them into a little water, in order to try whether he ld thereby make any farther diſcoveries, with reſpect to ſmall particles of fat; but to no purpoſe, only he ob- ved floating upon the water very ſmall particles of fat, t were coagulated in a ſpherical figure, and the very geſt of them was no bigger than a grain of ſand: He ced theſe particles upon a glaſs-plate, and viewing them h a microſcope, he obſerved the abovementioned figure, plain as before; and other fat particles ſeemed to be of a ferent figure.

Fig. 19. repreſents between IKLM one of the ſaid par- es, coagulated on the water, that was not very conformable h the other melted fat particles; for, all the particles did t melt nor were all extracted by the water, but coagulated reon in ſmaller and larger globular particles; and upon ing out the remainder of the thin ſlices of fat, that float on the water, and viewing them with a microſcope, he nd that ſeveral of the fat particles appeared entire to the ; and whereas they were before very ſmooth and even in ir ſides, they were now changed into rough and uneven rticles; ſo that one would be apt to think, that there were o different ſorts of particles in the fat, and that one ſort lted more eaſily than the other.

Now in order to get theſe melted particles of fat out of water, without altering them, M. *Leewenhoeck* made uſe a round glaſs, with which he ſkimm'd the ſurface of the ter; by which means ſome of the coagulated particles ck to the glaſs: Moreover, he again melted ſome of the particles, which had been coagulated upon the water,

over a coal fire, as they lay in the water; and when they were again coagulated, upon viewing them with a microscope, he found the small fat particles still smaller than those that were melted out of the water.

In this last observation he was surprised at the inconceivable number of veins and membranes, that were diffused thro' the fat, as also of the separated fat particles, involved in their several membranes.

After thus cutting off with a pair of scissars some pieces of the caul, or net spread over the hinder quarter of a sucking lamb, upon which there was little or no fat, and placing them before a microscope, he observed again, that the fat particles, where there were very few of them included between the membranes, were of a more globular figure than in other parts, where a good many lay together, and that in other places they were press'd or bruis'd, which he supposed was occasioned by the butcher's squeezing the caul in that place with his fingers; and in another place the fat particles had been so torn in pieces, that he could observe nothing remaining but the skins of the fat globules.

Moreover, he observed that the fat particles had such a pinch or dent in them, as M. *Leewenhoeck* shewed were in the globules of flower of wheat; from which he was confirmed more than before in his opinion, that the fat globules might be separated entirely or in part, from the pellicle with which they are surrounded, by opening the dents without breaking the pellicle.

Then he took off the thin membranes, which encompassed the fat particles, and viewing them with a microscope, he observed, that the fat particles had imprinted a roundish figure on the membranes, inclining to a hexangular one, which was an agreeable sight; but in other parts they were of an oval figure.

Moreover he took a flat fish, called a *plaise*, and taking off the fat which adhered to the vessels, or bones, he viewed it with a microscope and observed, that the fat particles were of several sizes; and some so small, that he judged, that 50 of the smallest were no bigger than one large fat globule; and moreover he observed, that several of the fat globules had such a dent in them, as we find in the meal or flower of those little white beans, called *French* or kidney beans.

M. *Leewenhoeck*, taking a little of the fat of a perch, which was 9 or 10 inches long, and viewing it with a microscope,

ope, could not discover any small particles therein, nor any ternal dent, as he had observ'd in the fat of a small plaise.

After that the fat of a perch had lain an hour or two upon e glass, he view'd it again and observ'd, that the particles ere become smaller; and that the pellicle of the fat parti- les, which was still beset with some, was shrunk or wrinkled, s it were; and the fat that had burst out, lay about the fat articles, and was so fluent and transparent, that no parts ould be discover'd therein.

From this observation M: *Leewenhoeck* began to think, whether each of these fat particles was not furnished with an rifice or hole, out of which the fat might be protruded at all imes, as often as the parts of the fish stood in need of nou- ishment, without laying quite open the pellicle of the fat lobules; for, we constantly find, that when the eggs of the erch, called the roe, increase in bulk, its fat decreases; yea, n such a manner, that when the said eggs are arriv'd at their tmost bigness, there is seldom or never any fat to be observ'd n the intestines of the fish.

An Account of a new Island rais'd out of the Sea near Tercera; *by Mr.* Forster. Phil. Transf. N° 372. p. 100.

JOHN *Robinson*, master of a small pink-snow from *Piscataqua* in *New England*, arriv'd at *Tercera Dec.* 10 1720; at 17 leagues distance, bearing S. E. from the said sland he observ'd a fire break out of the sea; *Dec.* 18. he got under sail at 12 o'clock at night and stood from *Angras* S. E. The next day at 2 o'clock in the afternoon, he made an island ll fire and smoke; he continued his course, till the ashes fell all night on deck like hail or snow; he bore from it, the fire and smoke roar'd like thunder or great guns; at break of day he stood towards it again; at 12 o'clock he had a good observation, two leagues south from it; he sail'd round it and so near, that the fire and matter it threw out, had like to have done him damage; in which consternation all betook themselves to prayers, being in danger of driving ashore; then a small gale sprung up at S. E. and carried them clear to their great joy; the breeze was accompanied with a small shower of rain, which caus'd a great dust to fall on deck; with the said breeze he stood away for *Tercera*: The Governor inform'd him, that the fire broke out *Nov.* 20. 1720. in the night, and that the prodigious noise it made, caus'd an earthquake, which shatter'd several houses in the town of

Angra

Angra and places adjacent. Prodigious quantities of pumice-ſtones and half broil'd fiſh were found floating on the ſea for ſeveral leagues round the iſland, and abundance of ſea-fowl hovering about it.

Mr. *Forſter* was inform'd by an acquaintance, that in his paſſage from *Cadiz* to *London* the latter end of *April* 1721, he obſerv'd the ſea from cape *Finiſterre*, almoſt to the chops of the channel, cover'd with pumice-ſtones, ſome of which he gave him.

The Effects of a violent Shower of Rain *in* Yorkſhire; *by Mr.* Ralph Thoresby. Phil. Tranſ. N° 372. p. 101.

THE daſhing (as they call it) of two large watery clouds upon the hills, occaſioned an inundation at *Riponden* near *Halifax*; whatever was the more immediate cauſe thereof, the effects were diſmal and ſo ſudden, that tho' it happen'd in the day-time, *May* 18. 1722, between the hours of four and five o'clock in the afternoon, the people could not ſave their lives. By the modeſteſt computation the *Beck* was rais'd two yards at leaſt in perpendicular height above what was ever known before, which may be eaſily conceiv'd by the ſituation of the place, implied in the termination *den*, which ſignifies a deep valley between pretty ſteep hills on each ſide. *Fontes in convallibus Pſ.* 103. 2. is in the *Saxon* verſion render'd ƿillaꞅ on denum; and *Valley of Tears, Pſ.* 83. 6. dene teaꞃa. Several houſes, four mills, (according to ſome ſix) 9 ſtone and 10 or 11 wooden bridges were carried down by the flood, and the wheels, dams and ſluices (or *goits* as they call them in *Yorkſhire*, from the verbal noun ᵹeotan *fundere*) of moſt of the mills, that were left ſtanding, broken and damaged, and a great deal of cloth gone: Fifteen perſons were drowned; ſeven out of eight in one houſe were either kill'd by the fall of it or drowned.

The rapidity of the torrent was ſo violent, that it took down the north ſide of *Riponden* chappel and carried off moſt of the ſeats. The Rector of *Caſtleford* inform'd Mr. *Thoresby*, that ſeveral goods were carried down ſo far, tho' upwards of 20 miles off. It tore up the dead out of their graves: It ſwept away all the corn-land as deep as the plough had gone: Some perſons ſav'd themſelves by forcing a way out at the roofs of their houſes, and ſitting upon the ridges, till the flood abated.

That

That day Mr. *Thoresby* had a ſmart thunder-ſhower upon ie moor, as he was coming home.

An Account of a new Experiment made with the Blood of a Perſon dead of the Plague; *by Dr.* Couzier. Phil. Tranſ. N° 372. p. 103.

ON the firſt of *April* 1722, Dr. *Couzier* took a quantity of blood from the body of a perſon dead of the plague, nd mix'd it with warm water; which mixture he attempted o inject into the crural vein of a dog, but the end of the yringe being two large to enter the vein, the experiment did ot ſucceed.

Upon this he laid ſome of the ſame infected blood upon e wound, and cover'd it with a dreſſing, which the dog ot off in the night; next morning he found the dog had k'd his wonnd, and that he refus'd his food; towards night e began to bemoan himſelf, and gave ſigns of approaching eath; next morning he found him dead, the wound being nſiderably ſwell'd and gangren'd, as alſo the edges round e ſwelling.

Upon opening the body, the liver was found ſomewhat rger than uſual, with ſpots of a livid purple, as in the dies of perſons dead of the plague: In the ſtomach was und a quantity of black coagulated blood, of the ſize of a n's egg; this in all likelihood was what he had ſwallow'd pon licking the wound. The heart was very large, with black grumous blood in the ventricles, and the auricles were rn'd blackiſh and gangrenous.

n Account of an Experiment made with the Bile of Perſons dead of the Plague; *by Dr.* Deidier. Phil. Tranſ. N° 372. p. 105.

TWO dogs were made to ſwallow a pretty large quantity of the bile taken from the bodies of perſons dead the plague.

Upon this the dogs appeared heavy and melancholy, refus'd eir food, and made water very often, eſpecially when they ere any ways diſturb'd; their urine was thick, and very tid, and their groſs excrements tinged with the black and eeniſh bile, they had ſwallowed. But in a few days oſe accidents went off, and the dogs perfectly recover'd.

Obſer-

Observatiens on a Fœtus, *and the Parts of Generation of a* Sheep; *by M.* Leewenhoeck. Phil. Tranſ. N° 373. p. 151.

AN ewe, which within two years time had twice lamb'd, happen'd to be cover'd by a young ram about 20 weeks old; about five days after the ewe was kill'd; and out of her belly 28 pounds of fat were cut; but obſerving, upon opening her, that the *uterus* was four times bigger than ordinary, the butcher brought it, together with the *ovaria* to M. *Leewenhoeck*, aſſuring him, it was not quite five days ſince the young ram had cover'd the ſheep, and that there was no other ram thereabouts.

M. *Leewenhoeck* began firſt to try to penetrate with the point of a ſmall pair of ſciſſars into the *uterus* from the *vagina*; but he found it ſo cloſe, that he could not; therefore, he cut a piece off from the *uterus*, out of which ran a clear water, and within it lay the *fœtus* with all its coverings; he ſpread this upon the back-ſide of a china tea-diſh, and finding that it ſtill contain'd more water, he made a ſmall inciſion to drain it, and to let it dry, that he might obſerve it the better; he could plainly ſee the *vertebræ* of the neck and back, as alſo the joints of its ſhort tail; he likewiſe thought he ſaw the eyes; but when it was quite dry, he could not obſerve its back-bone ſo well as before, when it was moiſt, tho' the deſigner ſaw the bones of the back very diſtinctly. M. *Leewenhoeck*'s deſign in drying it was to cut it in ſmall ſlices, in order the better to obſerve the internal parts; for, it was ſo exceeding ſoft and tender, when moiſt, that with the leaſt touch its parts would be diſorder'd and confounded: Therefore, he cut this *fœtus* into 15 ſlices and obſerv'd them with a microſcope, but could not be very poſitive about what he ſaw; he imagin'd he ſaw the inteſtines, as alſo the bladder; and coming to the breaſt, he fancied he ſaw the heart; but he obſerv'd (which was an agreeable ſight) two blood-veſſels that lay near each other in the brain, and how they were ſpread into branches.

Fig. 20. Plate VI. repreſents the *fœtus*, as it lay in its integuments; AB the *fœtus*; ACDEIK and AFGHLK the membranes in which it was involv'd, in the manner M. *Leewenhoeck* had ſpread and dried them, wherein the blood-veſſels are delineated as much as poſſible: As to blood-veſſels, they have, as he has often ſaid, no termination beſides

esides, they gradually become so exquisitely fine, that the lood which passes thro' them, can exhibit no red colour to ur eyes; so that there is no tracing them, when entering nto the vessels that return the blood back to the heart, expting in live animals, where one may observe the blood nter into the returning vessels: Before the butcher brought I. *Leewenhoeck* this *uterus*, he squeez'd it between his ngers, and told him he could feel nothing therein; and I. *Leewenhoeck* supposes he had done this several times, by hich means he tore off the vessels, by which the *fœtus* was stened to the *uterus*; and this he takes to have been the eason, that upon opening the *uterus* the *fœtus* with its coverngs came forth so easily.

MNOP Fig. 19. represents the *Tuba Fallopiana*; at P is he imaginary orifice, which is thought to suck the egg from he *ovarium*, according to the old absurd notion; at M is hewn, where the *tuba* increases in bulk, and at QR the eshy substance, which M. *Leewenhoeck* cut away from the *terus*. He then also had cut off the pretended *ovaria* and *va*, which latter were much too big to pass upon conception hro' the *Tubæ Fallopianæ*: He therefore, took the length f the *fœtus* with a pair of compasses, and measur'd it upon divided brass ruler, and this he also did as to its breadth; e then took the mean number between these and multiplied t twice by itself, to bring it to a cube number; next he took he length of the axis of an *ovum*, as it lay in the *ovarium*, nclos'd in its membranes, and taking the cube of that length nd dividing one cube number by the other, he found, that uch an *ovum* was about 7 times bigger than the *fœtus*, notvithstanding it had had near five days growth.

M. *Leewenhoeck* shew'd this *fœtus* with its integuments to wo physicians and a surgeon; as also the pretended *ovaria*, nd they agreed, that not one *ovum* was missing out of the *varia*. Upon asking them how it was possible such an *ovum* ould pass thro' the *Tuba Fallopiana*; one answer'd, that the *varium* was quite out of the question, and that it was nothing ut some fleshy substance: But the other said, that notwithstanding this, all animals proceeded from an egg, and the last old M. *Leewenhoeck*, that he believ'd the *tuba* to be neither hat of a sheep or lamb; but M. *Leewenhoeck* shew'd them t was from an ewe, that had lamb'd twice, and yet that the *uba* was neither thicker nor wider than that of a lamb.

After keeping these *ovaria* for some days, by which means they were pretty much shrunk in drying, M. *Leewenhoeck* had them delineated, in order to observe the bigness of the *ova*.

ABCDE Fig. 20. represents the *ovarium*, which was on that side of the *uterus*, in which the *fœtus* had lain: It is to be observ'd, that the *uterus* of a sheep is divided by a membrane, so that the *fœtus*'s cannot touch each other; DE the part where it was fastened; in this *ovarium* may be observ'd a round protuberance, beset with several others, and call'd an *ovum*: This *ovarium* is not represented so large, as it was, when M. *Leewenhoeck* cut it off from the parts it grew to; there was besides, at one side of that *ovarium*, a large round body, grown to the *ovarium*, which also seem'd to be an *ovum*; as FG Fig. 21; on which there appear'd several other small round bodies, protuberant therefrom. Now on the other side of the *uterus*, there was a large fleshy prominent *ovum* (as it is call'd) which might plainly be observ'd without a microscope, whose bigness is likewise represented at HI Fig. 22. upon which may likewise be observ'd a small round body, out of which other round bodies still smaller, appear protuberant. After he had quite dried these *ova*, represented in Fig. 20, 21, 22, he still observ'd more and more of the prominent round bodies thereon; insomuch that upon one of them, he told 16 round little bodies, some of which, by losing all their moisture, were sunk in and had a dent in the middle: Furthermore he cut these *ova* with a very fine sharp knife into thin slices, and then viewing them with a microscope, he observ'd blood-vessels therein, and likewise other sorts of vessels, which he did not take for blood-vessels; and among the rest one so big, that a hair of the head might enter it; besides a great many others exceeding small.

After several observations he could conclude in no other manner than that the pretended *ova* consisted of nothing but vessels, and that the superfluous moisture, convey'd to these *ova*, did not circulate (excepting only what was in the blood-vessels) and by overcharging the vessels rais'd them into these small protuberances, and sometimes bursting them, did thereby leave a dent in the middle; which dent having been observ'd by some persons, they firmly believ'd, that that was the place where the *ovum* was suck'd out, from whence sprung the *fœtus*.

Had

Had M. *Leewenhoeck* got this *uterus* without being squeez'd, e does not question, but that he might otherways have discover'd all the members of the *fœtus*, since he could plainly bserve its back-bone even with the naked eye, tho' not uite of five days growth.

After this it cannot be pretended, that the *fœtus in utero* at first no other than an unform'd mass.

bservations on the Callus *of the Hands and Feet; by the Same.* Phil. Transf. N° 373. p. 156.

N *September* 1719, M. *Leewenhoeck*, feeling an acute pain at the joint between the foot and little toe, which he magin'd to be owing to the more than usual thickness of the *llus* or hard skin upon that part; he caus'd his servant, artly with his nails and partly with a penknife, to take off at hard skin, and let it fall upon a blue paper.

This *callus* was compos'd of little scaly shivers, lying upon ch other; and the whole piece was as large as a small nail f a man's hand.

He view'd the said shivers thro' a microscope, but could ot satisfy himself; because they lay so irregular upon each ther.

Moreover, taking a little bit of the aforesaid *callus*, he laid on a clean glass-plate, steep'd it in pure rain-water, and ently dividing it with a piece of a quill, he was amaz'd to e into what a vast number of particles it separated, and at with as much readiness, as if they had never been in'd.

Afterwards he took two or three of the said particles, veral of which were of the figure of a weaver's shuttle, road in the middle and pointed at each end with a line in e middle, like those on the outside skin of fruits or of the uman body, but generally irregular; they were very thick proportion to their bigness: Having laid two or three of e said particles upon a clean glass, and putting a drop of ater to them, as large as a coarse grain of sand, and divid-ng the same as much as possible; upon viewing the divided articles thro' a microscope, he was surpris'd at the prodiious number of exceeding small particles, which occur'd to is sight and were of the same figure, as above-mentioned.

Moreover, he took some of the thickest pieces of the horny in, but not half so thick, as the back of a small knife, and itting them into as thin slices as possible, he placed them

upon a clean glaſs, in order to diſcover the exceeding thin particles that lay upon each other; and having moiſtened them, they ſpread themſelves out farther, and when they became dry again, they ſeparated into ſeveral parts; and he likewiſe obſerved, that each of theſe ſeparated parts were compoſed of ſeveral thinner particles, lying on each other.

In order to have a clearer notion of the contexture of thoſe particles, by which the ſkin of the hands and feet of ſuch, as are inured to hard labour, or walk much, increaſes in thickneſs and is ſurrounded, he cauſed a ſmall portion of the aforeſaid ſeparated particles to be delineated, as ABCD Fig. 23. Plate VI. tho' they were not all ſo exact and compleat, as repreſented in the Fig. and according to the beſt of his obſervation, they had all been ſeparate pieces and none of them united to each other.

Again he placed before a microſcope ſeveral ſmall pieces of this hard ſkin, he had cut off at the thickeſt part thereof and moiſtening them with fair rain water, he put them upon the glaſs-plate, by which means they were pretty much dilated; and being dried, they ſhrunk again and thereby appeared in ſeveral oblong particles, each of which ſeemed to conſiſt of other oblong particles, as repreſented at EFGH Fig. 24; ſo that EH or FG was the thickneſs of that piece of thick ſkin, which he had cut thro'.

From this obſervation, he conſidered, whether one of theſe long ſtripe-like particles, ſuch as appeared to the eye at EF or HG, might not be the thickneſs, which the horny ſkin had acquired in the ſpace of a month; and whether the very thin particles, which appeared in ſuch a ſmall ſtripe were not the accretion, the hard ſkin received in a day. The laſt mentioned ſmall piece repreſented Fig. 24. was not ſo large to the naked eye, as a common grain of ſand. And whereas he placed before a microſcope another very ſmall piece of ſkin, ſomewhat thinner than the former, he could perceive the exceeding thin particles, repreſented by IKLM Fig. 25, which were the *ſtrata* or beds, in which the horny particles of the ſkin lay, and ſo compoſed its thickneſs.

For his farther ſatisfaction he cut out of the hand of a maſon, a laborious workman, where it was moſt thick and brawny, two bits of the hard ſkin; and then ſlitting it into ſmall ſlices, he obſerved eaſily enough the thin particles lying upon each other, but could not ſeparate the ſmall ſcales of which each little *ſtratum* of the hard ſkin conſiſted

becauſe

because, as M. *Leewenhoeck* imagined, the particles of the said skin, thro' the hard working of the man, were so press'd upon each other and so closely joined, that they could not be separated.

Now as the hands of masons or bricklayers are often covered with the sharp salts of the chalk or lime, which might prevent the separation of the very small parts of the hard skin; M. *Leewenhoeck* pared off some of the brawn from the hands of a carpenter, a diligent workman, and he found them as soft in the palms, as if he had never been used to labour; upon which M. *Leewenhoeck* asked him, whether he did not wash his hands very often; to which he replied, 10 times a-day at least. M. *Leewenhoeck* cut out two pieces of horny skin from the hands of a ploughman, and these he cut into little bits; but they were so hard, that a sharp knife, which he used, got several notches in doing it; and he observed, that the uppermost part of the skin was full of little rents, and all the *strata* pressed so closely together, that he could make no discoveries therein, only that the little beds lay upon each other, and that the thickness of the skin consisted thereof.

He put the two pieces of hard skin into warm water, in order to soften them and separate the parts from each other; but could not do it, because they were so strongly joined together.

In washing his hands, M. *Leewenhoeck* several times observed, that when he rubbed his palms strongly against each other, with very little water between them, some particles would be rubbed off from the skin, and continue between his hands.

For his farther satisfaction in this matter, he put one of his fingers into fair rain water, and with that washed the part of his thumb, which is joined to his hand; after which he rubbed both finger and thumb hard against each other, then he gently scraped with a penknife the matter he had loosened from the skin by moistening and rubbing it; and taking off a little of that which stuck to the knife, he put it upon a clean glass-plate, and setting it before a microscope, he saw with astonishment the great number of particles of skin, which lay scattered upon the glass, ; but more irregular than those he separated from the brawny skin of his foot, which were not very closely pressed together, because he did not walk much.

After

After this he moiſtened the back of his hands, and then rubbing them 10 or 12 times againſt each other, he ſcraped off very gently with a penknife ſome of the matter he had looſened by rubbing; and placing it on a clean glaſs, he viewed it with a microſcope, after having ſeparated the particles of the ſaid matter from each other with a little water, and he diſcovered a vaſt many ſmall ſcales which come off our hands.

As the ſkin of his hand was in no part thicker than that upon his thumb next the nail, having chiefly uſed his thumbs in the examination of microſcopical objects, he moiſtened one of his thumbs a little and rubbed the other againſt it, and placing before a microſcope the matter, thus rubbed off, he obſerved ſuch a prodigious number of particles (like thoſe repreſented by ABCD Fig. 23. but all irregular) as no man can conceive without having ſeen it.

Now as we find, that ſuch a quantity of particles is ſeparated from the hands, and daily renewed in a well conſtituted body, we muſt conclude, that in our bread we eat ſeveral of the ſaid particles thus rubbed off, and that they turn to nouriſhment; and M. *Leewenhoeck* is of opinion, that there is hardly any food prepared for us, eſpecially ſuch as paſſes pretty much thro' the hands, but that ſome of the particles thus rubbed off are mixed therewith; eſpecially kneading dough from meal or flower; and ſtill more, when the bakers knead with their feet, as in making rye-bread.

Since theſe obſervations concerning rubbing of his hands, M. *Leewenhoeck* took more notice thereof, when he waſhed and dried them than he did before; and he was ſurpriſed at the great number of particles that daily ſeparated from his hands and grew on them again; and at the particular proviſion, made for producing theſe particles in the palms of our hands and ſoles of our feet; whereas we do not by far meet with ſuch a quantity of particles, conſtantly produced in other parts of our bodies: For, if we obſerve ſuch as work much with the back of their hands, we ſhall not meet with any of that hard ſkin abovementioned, but only a kind of tumour or riſing, as the dry-ſheerers, or ſuch as dreſs cloth, have upon their left hands.

In ſhort, the manner of the production of theſe ſmall particles will be a myſtery to us, tho' our hands and feet muſt be fortified with ſuch a matter, to enable them to ſupport all that force and preſſure, they are obliged to undergo.

Of

Of the Motion of running Waters; *by Dr.* Jurin. Phil. Trans. N° 373. p. 179. *Translated from the* Latin.

S. *Michelotti*'s animadversions in his book *de separatione fluidorum in corpore humano*, on a dissertation of Dr. *Jurin*'s on the *Motion of running waters*, published in *Phil. Trans.* N° 355, are partly owing to his not throughly understanding the drift of that dissertation and partly that some things are not put in so clear a light as they are capable of: To obviate which, the Dr. first explains, what is to be understood by *the motion of water, running out at a hole in the bottom of a vessel*: For, there is a wide difference between the motion, or the quantity of motion of water, running out at a hole in a vessel (which motion is in a compound ratio of the quantity of water running out at the hole in any given time, and of the velocity, with which it runs out) and between the motion of the whole quantity of water or cataract of water, descending within the vessel towards the hole, and immediately about to flow out; this motion being in a ratio of the sum of all the products of each particle of water, constituting the cataract, multiplied into their respective velocities. The Dr. observing that one of these motions was often taken for the other, had a mind to illustrate the latter in his said dissertation, and bring it to a calculation, and apply it to the fluids in animal bodies.

Since, therefore, this motion was what the Dr. always meant by *the motion of running waters*, as plainly appears by all his propositions, he thought he might justly alledge, *that this motion had not hitherto been defined by any one, as far as he knew*; no mathematician having even so much as hinted at it: Wherefore, the Dr. is surprised, that neither *Michelotti* nor *John Bernouilli* were aware that in the preface to that dissertation (so often cited and so much censured by *Michelotti*) he did not so much as mention the velocity, with which water runs out at a hole, much less the velocity determined by *Bernouilli*.

In order to define the aforesaid motion, the Dr. needed no other than his third general *Theorem*: But since he thought, the property of the *Newtonian* hyperbolic curve, in which Sir *Isaac Newton* forms the cataract of the descending water, not unworthy the consideration of Geometricians, he would by the bye premise some things about it, as taken from *Prop.* 36. *lib.* 2. *Princip. Mathem. Philos. Nat.*

For,

For, it is plain to any one, who attentively considers Sir *Isaac Newton*'s proposition, that such a cataract should be formed by water descending freely, and accelerated in the manner of all other heavy bodies, without any other water surrounding it. And even if the cataract of water be surrounded with a hollow crust of ice, exactly answering to its figure, and by reason of its extreme politure making no resistance thereto; the cataract of water will not in the least press upon the ice, but only touch it and descend very freely; whence no alteration will be produced either in the figure, or velocity of the descending cataract. But if the circumambient ice be dissolved, there is no manner of occasion for such a strong battery as *Michelotti* p. 128, 129, 130, and likewise *John Bernouilli* have raised to break down the Dr's slender cataract; since Sir *Isaac Newton* himself has quite dissolved it, when he says, *Princip.* p. 304. *Liquescat jam glacies in vase*, &c.

The Dr. does not deny, but that there is some difference between the case, as laid down by Sir *Isaac Newton* and by himself; for, the cylinder of ice, which the former supposes to descend with a given uniform velocity and to dissolve, as soon as it touches the surface of the water, contained in the vessel, to the end, that the vessel may be always kept equally full, the Dr. has omitted and instead thereof has supposed an infinite surface of water, that by that means he might represent the whole solid or hyperbolic cataract: Yet this position alters nothing either in the velocity or motion of the running water.

What S. *Michelotti* says, p. 127. *that the Dr. begs the question*; and a little lower, *that the question, therefore, ceases and that the whole demonstration becomes an hypothesis*; this the Dr. does not understand: For in *loc. citat.* the question was not about the velocity of the effluent water, nor was there any demonstration adduced to prove that velocity; but the only thing the Dr. intended was from that velocity, to deduce the equation of the *Newtonian* hyperbolic curve; for, the Dr. had already determined the velocity of the effluent water, or rather assumed it, to wit, by supposing what Sir *Isaac Newton* had done, *viz.* that water by the force of gravity falls freely and in falling is accelerated.

Here the Dr. takes notice of an error, *Michelotti* p. 112, 113, would rashly fasten on Sir *Isaac Newton*, *Huygens* and Mr. *Keill*; *viz.* that they have supposed the force, by which the

whole

whole motion of the effluent water may be produced, equal to the weight of a cylindrical column of water, whose base is the hole, and height, double that of the water, contained in the vessel: This Sir *Isaac Newton* had briefly but perspicuously demonstrated in the second *Coroll.* of the aforesaid *Prop.* And another demonstration of it might be deduced from considering the entire hyperbolic cataract, which is equal to this cylinder and whose entire weight is spent on the descent of the water; but that is unnecessary, since the same thing follows very evidently from *Bernouilli*'s own proposition, which *Michelotti* so often commends and so strenuously defends. This will easily appear, if laying aside for a little the consideration of the column of water, incumbent on the hole, and making the calculation, he would, from the mass of water, running out at the hole in any given time, and from the velocity, with which *Bernouilli* has determined water to run out, determine the motion of the water and then find the weight, which, by the force of gravity, falling freely in the same given time, may produce the same quantity of motion; he will find this weight equal to that of double the column of water, incumbent on the hole, just as Sir *Isaac Newton* had determined it in the aforesaid *Corol.* But the same weight, suspended at one arm of a balance, will be kept in *æquilibrio* by the *impetus* of the water (at its very first efflux out of the hole) impinging in a continued stream upon the other equal arm of the balance and falling down, immediately after the impulse; which will easily appear upon making the calculation.

Dr. *Jurin* would here also remove a prejudice *Michelotti* p. 113. and others labour under: Sir *Isaac Newton* had demonstrated *Prop.* 37. *lib.* 2. *Princip. first edit.* that water runs out at a hole in the bottom of a vessel with that velocity, with which it might rise to half the height of the water, contained in the vessel. Experience is said to contradict this, by which it is discovered, that the effluent water rises to the whole height; and Sir *Isaac Newton* himself in the solution of the same problem *Prop.* 36. *lib.* 2. *the second edit.* ascribes to the water that velocity, with which it might rise to its whole height; consequently, he seems to contradict himself: But if this matter be accurately and judiciously considered, it will be found to agree very well both with Sir *Isaac Newton*'s first and second solutions, and likewise with experience; for, in his second solution, he supposes the jet of water narrower in

diameter, at a ſmall diſtance from the hole, than in the hole itſelf, in the ratio of 21 to 25; therefore, the ſection of the jet at that diſtance is to the hole itſelf, as 21×21 to 25×25, that is, as 1 to $\sqrt{2}$ nearly: And ſince the ſame quantity of water flows in a given time, either thro' the ſection of the hole, or that of the contracted jet, and conſequently, the velocities of the water in theſe ſections are reciprocally as the ſections themſelves, the velocity of the water in the hole will be to the velocity of that in the contracted jet, as 1 to $\sqrt{2}$; conſequently if the velocity in the contracted jet be ſuch, as that the water can riſe to the whole height of that, contained in the veſſel, the velocity of the water will not be greater in the hole itſelf, than what can raiſe it to half the height: Theſe two ſolutions therefore are conſiſtent with each other and experience: For, if by either of the ſolutions, from the given velocity, with which water is ſuppoſed to paſs either thro' a hole, or contracted jet, by calculation, the quantity of water to run out be found; the ſame will be found to agree nearly with the quantity of water, diſcovered by experiments; and even Sir *Iſaac Newton*'s own experiment, taking a hole, whoſe diameter is $\frac{5}{8}$ parts of an inch, agrees with this calculation; as alſo ſeveral other experiments with holes of ſmaller diameters, made at *London*: It is true, the experiments made by the accurate *Polenus* differ ſomewhat from theſe; but yet they give a leſs quantity of water, than according to this calculation, and never a greater, becauſe, probably, the veſſels were narrower in proportion to the ſize of the holes.

There ſtill remains one animadverſion more, or rather ſcruple p. 101, 102, ariſing hence, that in *Corol.* 17. *Theor.* 3. of the abovementioned diſſertation, Dr. *Jurin* had ſuppoſed the motion or *impetus* of the blood to be greater in all the capillary arteries taken together than in the *aorta* itſelf: To explain this, *Michelotti* would faſten on the Dr. an hypotheſis of a greater denſity of blood in the capillary arteries than in the *aorta*. Dr. *Jurin* diſowns any ſuch hypotheſis, having deduced the *Corol.* from the foregoing *Theor.* which treats of the motion of water running thro' any full pipe: Whence it appears, that the blood is no otherways conſidered in his *Corollaries*, than as it is fluid, and reſembles water: But *Michelotti*'s ſcruple appears to proceed from his taking the *impetus of the blood to be the quantity of its motion, produced by multiplying the velocity into the maſs, running thro' in a given time*;

time; which is quite different from Dr. *Jurin*'s motion or *impetus*; he having in that *Theorem supposed it equal to the motion of a mass of water, which runs out of a pipe in any given time, and whose velocity is such, as in the same time to run over a space equal to the length of the pipe.* The aforesaid *Corol.* easily flows from this *Theorem*; since in a given time an equal mass of blood runs thro' the *aorta* and capillary arteries, and the length of the tube, consisting of the *aorta* and capillary arteries, is greater than the *aorta* alone. This the Dr. has the rather observed, because not only *Michelotti*, but other mathematical writers in treating of forces, which either put into motion the liquor, contained in pipes, filled therewith, or stop its efflux, consider only the mass and velocity of the effluent fluid, without regarding the length of the pipes: For, *cæteris paribus*, a fluid is with greater difficulty either thrown out of a full pipe, or stopped in its efflux, the longer the pipe is; since the whole mass of fluid, contained in the pipe, must be put into motion, before any part thereof can flow out at the orifice; as also the entire mass be necessarily stopped, to hinder the efflux of any part thereof just ready for it.

'The principles of *Bernouilli*'s demonstration of the velocity of water running out of a hole of a full vessel, are, 'that the lowest drop of the liquor, or that immediately incumbent on the hole of the vessel, is considered, as pressed 'upon, or (as he calls it) animated by a certain accelerating 'gravity, which is to the natural gravity, as the height of 'the water, or of the whole liquor, incumbent on the hole 'of the vessel, to the height of the small drop; that is, as 'the absolute weight of the column of water, insisting on 'the hole of the vessel, to the absolute weight of the drop: 'For thus, nothing remains, but to find how great a velocity 'the drop, animated by that greater gravity, may acquire, 'when it falls thro' a line, equal to its height; that is, after 'it has got quite out of the hole: For it is pressed upon by 'the whole column of water, consequently, animated by 'the greater gravity, so long as any of the drop (which he 'supposes a small solid column) remains above the hole.'

The weakness of this foundation appears thus; since *Bernouilli* makes use of nothing to animate, as he calls it, the lowest drop with the aforesaid accelerating gravity, but pression alone, or the weight of the column of water insisting on the hole: Let all the water, surrounding that column, be sup-

poſed to be frozen, and the column to fall without any reſiſtance along the ſmooth ſurface of the ice; then as long as the hole is ſhut up, the ſmall drop next the hole will be preſſed upon by the whole weight of the incumbent column of water, in the manner *Bernouilli* ſuppoſes.

Now let the hole be opened, and a free exit be given the water. What will then be the conſequence? ***Will the loweſt drop be urged or animated by the accelerating gravity, which is to the natural gravity, as the height of the whole water incumbent on the hole, to the height of the drop?*** By no means; but it will be urged only by its own natural accelerating gravity: For, as ſoon as the loweſt drop begins to deſcend, tho' with an infinitely ſmall velocity, it will no longer be urged by the weight of the incumbent column of water: For, it is impoſſible for the column of water to preſs upon the ſubjacent drop without being hindered in its deſcent by that drop: But it is not hindered, becauſe it does not endeavour to deſcend with greater velocity than the loweſt drop tends downwards by its own force of gravity; but the column and drop deſcend equally, ſo that the drop will neither quit the column, nor receive any force or preſſion from it.

The Dr. therefore, thinks *Bernouilli*'s demonſtration falls to the ground: His miſtake ſeems to be owing to his not adverting to the difference, between a body, preſſed upon by an incumbent weight, when that weight is only urged by the natural accelerating force of gravity, and a body impelled, or animated (as he calls it) by the accelerating force of gravity, preternaturally encreaſed. In the latter caſe, the body will deſcend with a greater velocity than what can be produced by the natural force of gravity, according to *Bernouilli* himſelf: But in the former caſe, however the body, preſſed upon, while it is at reſt, may be urged by the incumbent weight; yet as ſoon as it begins to deſcend, it will do ſo entirely with the ſame degree of velocity, as if it were preſſed upon by no incumbent weight at all.

To illuſtrate this by an example; ſuppoſe a ſolid column conſiſting of 100 pieces of gold, laid upon each other, at reſt upon a table, and the loweſt piece preſſed upon by the weight of the reſt; now if a hole be made in the table, under the pieces, that the undermoſt may ſlip thro'; as ſoon as it begins to fall, it is immediately freed from the weight of the incumbent pieces; and then the undermoſt piece and all the reſt will deſcend with the ſame velocity, as if there were only that loweſt piece upon the table.

If from the velocity, with which, according to *Bernouilli*, water runs out at a hole, and from the mass of water (as determined by that velocity) running out in any given time, any one would determine its motion, he would find it twice greater than what could be produced by the force of gravity, from the weight of the column of water, incumbent on the hole.

The Dr. recommends the two following experiments (in order with more certainty to determine the controversy) either to be tried a-new, or at least diligently considered: The one is Sir *Isaac Newton*'s described p. 305. *Princip. second edit. viz.* From the mass of water, running out in a given time, to find the velocity with which it passes thro' the hole: The other is *Mariotte*'s in his book *de mouvement des eaux par.* 2. *disc.* 3. *regl.* 1. and made with a cylindrical pipe, open at both ends, its lower part reflected upwards and full of water: Whence it may be easily estimated, whether the first drops of effluent water can rise to so great a height, as *Bernouilli*'s demonstration requires.

A remarkable instance of the Infection of the Small-pox; *by the Same.* Phil. Transf. N° 373. p. 191.

A Young gentleman, ill of that sort of small-pox, called the coherent, or intermediate species between the distinct and confluent kind, on *Wednesday* the 3. of *October* 1722, being the 6. day from the eruption, grew delirious in the night, and got out of bed in spite of two nurses that attended him; and seising one of them by the neck between his bare arms, he pressed her forehead against his naked breast, then covered with the small-pox, in the state of maturation, and held her for some time in that posture: She was heated by striving with him and in struggling to get loose, she was sensible, that she bruised and broke some of the pustules with her forehead: This woman was about 40 years of age, of a clear, florid, sanguine complexion: She told the Dr. she had had the small-pox, when about 7 or 8 years of age, and had been pretty full of them, tho' there were no marks on her face: On *Friday* morning the small-pox began to appear upon her forehead, and increased by degrees to between 50 or 60: She had likewise a few pustules on the back part and sides of her neck, where the patient had grasped her with his naked arms; but had none, as she told the Dr. on any other part of her body: The lower part of her face was entirely clear of them, and those upon her forehead were chiefly confined to the middle and most prominent part of it, that had been

been preſs'd againſt the patient's breaſt: They roſe gradually and came to maturity, in the ſame manner as the ſmall-pox of the milder coherent kind, with a great inflammation and ſwelling of her forehead, and the adjoining part of her face; eſpecially, between the eye-brows, where a ſmall cluſter of the puſtules were ſeated; inſomuch, that on the 9. of *October* her right eye was quite cloſed up, and the left almoſt in the ſame condition: But all this time ſhe had no fever, ſickneſs or other ſymptom of the ſmall-pox, beſides this eruption, and the inflammation and pain that attended it. That night ſhe cauſed a bliſter to be applied to her neck; upon which ſhe recovered the ſight of her eye the next day, being the 6. from the eruption, when the puſtules were turning and beginning to ſcab. The ſcabs agreed with thoſe of the milder coherent ſort in their appearance and duration. The Dr. ſaw her ſeveral times after this; particularly on *Monday* the 22. of *October*, which was the 18 day from the eruption of the puſtules, when ſhe had ſtill ſome ſmall part of the ſcabs, remaining on her forehead.

In this inſtance it is worthy remark. 1. That this woman, tho' ſhe had had the ſmall-pox before, was notwithſtanding infected again by the immediate and cloſe application of the variolous matter to her ſkin, when her body was heated with exerciſe: Which ſeems to prove, that ſuch an application is more effectual to give the infection than the bare morbid effluvia, ariſing from the body of the patient, and received into the ſound body by inſpiration: For, that ſhe received no infection by inſpiration is plain, from the appearing of the ſmall-pox upon thoſe parts only where there had been ſuch an application and contact: From which it appears very probable, that a perſon who has already had the ſmall-pox, as the man, inoculated by Mr. *Tanner* in St. *Thomas*'s hoſpital, may poſſibly receive it again in ſome ſlight degree by inoculation; that being ſtill a more cloſe and immediate application of the variolous matter to the blood, and juices of the ſound perſon, than when it is applied only by contact to the ſkin, whole and unwounded.

2. That the infection, communicated to this woman, not being univerſal, as appears from her having no fever or ſickneſs, or general eruption of the puſtules all over her body, but only on the parts, infected by immediate contact; no argument can hence be drawn, for a perſon's being liable to undergo the ſmall-pox a ſecond time; ſo as to have the uſual ſymptoms of that diſeaſe, and a general eruption of the puſtules; but rather the contrary.

3. That

3. That the time, in which this infection ſhewed itſelf, by the
pearance of the puſtules, is very different from that obſerved
on inoculation; the firſt appearing in about a day and a half;
hereas in the latter caſe, the eruption generally ſhews itſelf on
e 10. day or not above a day ſooner or later, as appears from
e accurate and curious obſervations of Dr. *Nettleton.* Which
fference is what ought in reaſon to be expected; ſince in one
ſe the infection went no farther than the parts affected by im-
ediate contact; whereas, in the other it muſt be propagated
ro' the maſs of blood to all the parts of the body.

n *Account of two Obſervations upon* Cataracts; *by* S. Antonio Benevoli. Phil. Tranſ. N° 373. p. 194.

ON the 13. of *July* 1720, S. *Benevoli* couched a *German* ſoldier of cataracts in both his eyes, who immediately er the operation recovered the ſight of them and continued to till his death, which happened of an acute illneſs on the 6. *April* 1722. Upon this S. *Benevoli* took the eyes out of eir orbits, in order to examine, whether the cataracts, conſted of a membranous pellicle, as ſome writers affirm; or of a eternatural opacity in the chryſtalline humour, as others pre-nd. Proceeding immediately to the diſſection of the left eye, pon a careful and very exact examination of all its contents, could not find any ſuch thing as a pellicle within it; but diſvered a ſmall yellowiſh body at the bottom of the bulb of the e, of a lenticular ſhape, without adhering to any of the parts the eye; which, upon extracting it, appeared to be the chryalline humour grown opaque and ſomething leſs than its natul ſize, having two or three ſmall dents or impreſſions, made in s circumference, which it had received from the needle, in the peration of couching.

The next day he examined the right eye in the ſame manner, nd he diſcovered therein the chryſtalline humour grown opaque nd depreſſed, in the ſame manner as the former, to the bottom f the eye; ſtill evidently retaining the marks of the needle; ut he found no pellicle within the eye, notwithſtanding the rictest ſearch he could make.

S. *Benevoli*, having formerly made experiments on the eyes f dead ſubjects, at *Bologna*, in company with Dr. *Valſalva*, ntroduced the needle into the eye in the ſame place and in the me manner, as is commonly practiſed in the operation of ouching; and having afterwards diſſected the ſame eyes, he lways found, that the needle had paſſed into the eye on the back-

backside of the chrystalline humour; so that it was impossib
to bring the needle forwards from thence into that part of th
aqueous humour, seated between the *uvea* and chrystalline hu
mour, in order to depress a pellicle, seated there, according t
the common opinion, unless he had passed his needle thro' th
substance of the chrystalline humour.

This curious author likewise observes, that the aforesai
space, between the *uvea* and chrystalline humour, is so ver
narrow, that tho' he finds it not impossible to introduce a needl
into that space; yet there is by no means room enough to tur
the needle up and down in all directions, with that freedom
used in couching cataracts, without wounding either the *uve*
or chrystalline humour.

Lastly, S. *Benevoli* observes, that in his practice of couchin
cataracts for several years (having generally couched about 1
or 14 in a year) he had always found, that he worked upon
hard and resisting substance, which being tenderly touched b
the needle, would vibrate and fluctuate backwards and for
wards, and sometimes return against the needle with a sensibl
impetus, which by no means agrees with the common notion o
the cataracts consisting in a pellicle or membranous substance.

An Observation of a solar Eclipse *at* Greenwich *Nov.* 2 *1722. p. m. by Dr.* Halley. Phil. Transf. N° 374. p. 19 *Translated from the* Latin.

h	′	″	
1	29	16	DR. *Halley* observed the eclipse just begun.
1	31	6	The distance of the cusps 7′ 4″; whenc the part eclipsed 47″ and the true begin ning was at 1^h 28′ 58″.
1	34	18	The distance of the cusps 10′ 50″.
1	42	28	Repeated 16′ 20″.
1	43	26	The inclination of the cusps to the right 44° 30
2	32	37	The remaining clear parts 17′ 20″.
2	40	16	Again repeated in the middle of the eclips nearly, 17′ 9″.
3	28	45	The inclination to the left 19°.
3	31	45	The distance of the cusps 15′ 21″.
3	37	35	Again repeated 10′ 50″.
3	43	25	The end of the eclipse doubtful, by reason th sun's limb was uneven and undulating and no well defined.

′	″	
43	45	The eclipſe certainly ended.

The ſky was all the time ſerene and calm.

he ſame Eclipſe *obſerved in* Fleetſtreet, London; *by Mr.* George Graham. Phil. Tranſ. N° 374. p. 198.

	h	′	″	
M.	1	28	38	Apparent time, the eclipſe began.
	2	29	34	By eſtimation, the cuſps parallel to the horizon.
	3	43	22	The end.
	2	14	44	The duration.

he quantity eclipſed five digits $\frac{716}{1000}$.

Mr. *Graham* had very correct obſervations both of the ſun d ſtars, the 26, 27, and 28. *Nov.* for determining the exact ne by his clock.

For ſome minutes before the eclipſe began, he obſerved the with a 12 foot teleſcope, furniſhed with a micrometer, eping that part of the limb in the middle of the glaſs, where expected the moon firſt to touch; and in leſs than four ſends of time, from the moment he judged the eclipſe began, it s ſo conſiderably advanced, that he could not doubt of havg the beginning to leſs than three ſeconds: He believes the act time of ending was within the ſame limit, notwithſtandg that the undulation of the limb was then much greater than the beginning: At the time of the greateſt obſcuration, the rts eclipſed, meaſured with the micromer, were 927 ſuch rts, as the ſun's vertical diameter contained 1946, which was en a little before the beginning of the eclipſe.

The ſky was clear and free from clouds, till near the end, en a narrow cloud obſcured ſome part of the ſun's diſk; but t part of the limb, where the eclipſe ended, continued clear after it was over.

By this obſervation the beginning did not differ 2′ and ½ and end not ½ a minute from Dr. *Halley*'s computation; and if Dr's computation, which was made for *Greenwich*, had en reduced to the meridian of *London*, the difference would ve been ſtill leſs.

The ſame eclipſe was obſerved by Mr. *Hawkins* at *Wakefield Yorkſhire*, to begin at 1^{h} 21′ *p.m.* and to end at 3^{h} 30′ 3″. e ſun's diameter was obſcured upwards of five digits.

An Account of the Particles and Structure of Diamond *by M.* Leewenhoeck. Phil. Transf. N° 374. p. 1 *Translated from the* Latin.

AFTER M. *Leewenhoeck* had discover'd, that so metals, and even grains of sand, consisted of very sm particles of the same matter, he turn'd his thoughts to c sider a diamond, as whether it also consisted of the sa sort of particles, that might be observ'd with the mic scope.

M. *Leewenhoeck*, therefore, viewing with a microsc a small diamond, could observe with the naked eye a gr many particles in the unpolish'd and dark part thereof; he found, that it consisted of small particles: But not be satisfied with these observations, he resolv'd to break diamond into pieces, in order to consider these pieces.

Laying, therefore, a diamond upon a hammer, he stru it once and again with another hammer, and thereby it broke into four or five pieces: With which not being satisfied, he had a mind to break the diamond into smaller bits, and accordingly he folded up a piece, big than the rest in a double piece of paper, that he might none of it.

Here M. *Leewenhoeck* was surpris'd at the hardness o diamond; which tho' often struck upon with considera force, broke only into four or five pieces without any sm bits.

Upon placing the last mentioned pieces before a mi scope, he view'd them, and found that all of them consi of very small particles: And upon exposing them to rays of the sun, he observ'd a kind of little flame break from them, and bigger than any he had ever observ'd.

He observ'd with his naked eye one small piece that its flat and square crack, directly expos'd to the sun; an far as he could judge with his naked eye, three or four h of a man's beard in breadth.

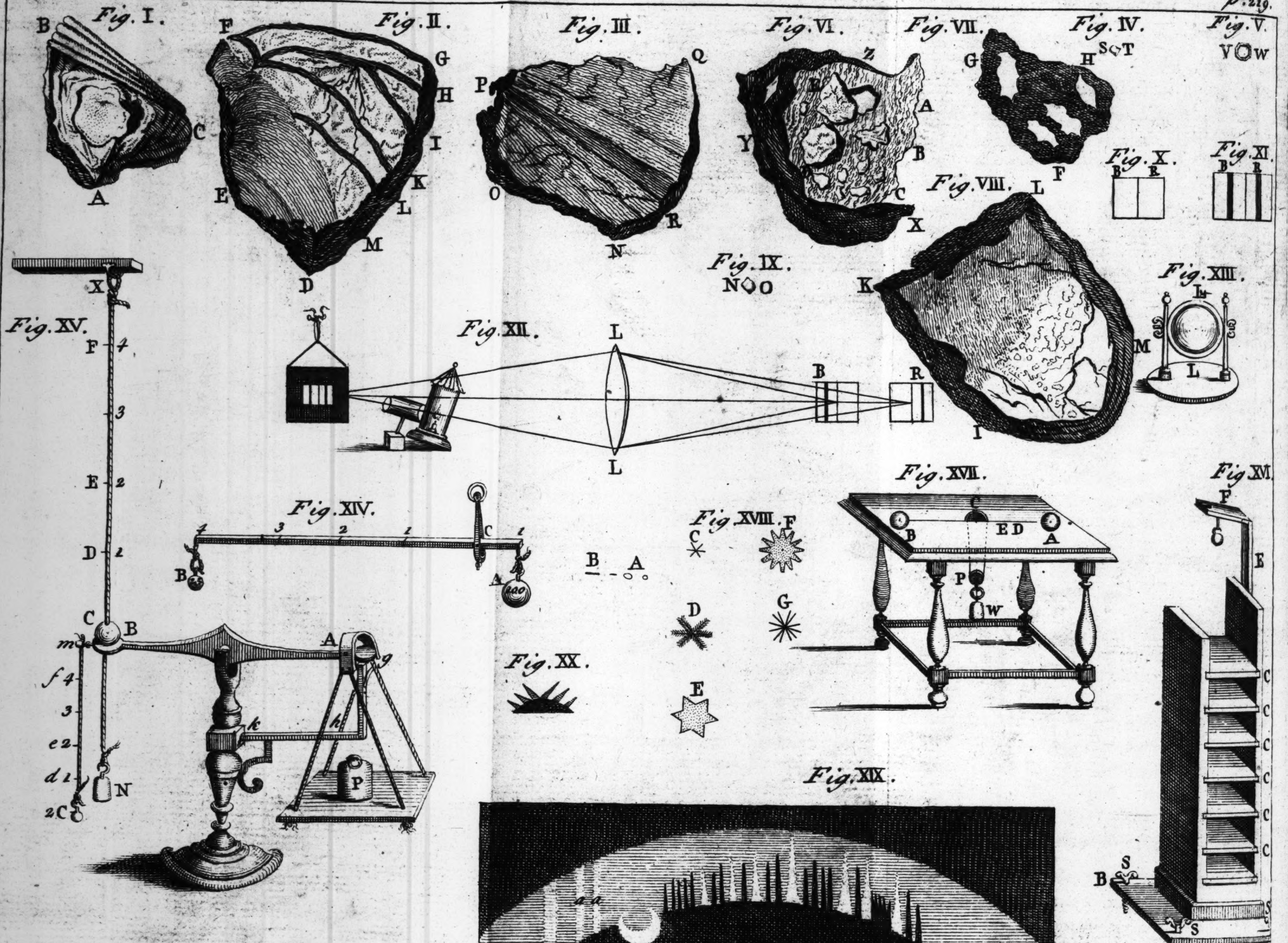
Such a quantity of sparkling flames issued from piece of diamond, that he judged them upwards of 400 few of those flames lay closer to each other and were big than the rest: Whence he concluded, that the particle the diamond were bigger in that place and more regul dispos'd than the other particles.

A

Vol. VII.

PLATE. VII.

p. 219.



G. Smith del.

Hulett Sculp.

Afterwards he view'd another piece of diamond, of about the ſame bigneſs with the former, which was alſo directly expos'd to the rays of the ſun; and he found it conſiſted of the ſame number of very ſmall particles: From the half of that little piece, there likewiſe aroſe the ſame ſparkling flames, but ſmaller; and in the other half a kind of waving flame was obſerv'd, with a continual coruſcation, like a faint lightning.

Moreover, after withdrawing theſe little pieces out of the ſun's rays, various appearances ſtill preſented themſelves to his eyes; among other things a little flame ſeemed to dart aloft from each particle of the diamond.

Moreover, he had nine ſmall pieces of diamond lying before his microſcope; and in ſeven of them he obſerv'd theſe particles, which ſhot forth the ſparkling flames as was already mentioned; and in two others he could likewiſe obſerve thoſe particles, of which he ſuppoſes a diamond is compos'd; but they had their planes turn'd towards the ſun, in ſuch a manner that he could diſtinctly obſerve ſeveral particles at the ſame time.

It was an agreeable ſight to behold ſo many appearances of ſparkling flames, moſt of them of a bright flame-colour, and ſome greeniſh: M. *Leewenhoeck* was ſurpris'd to obſerve at the extremity of ſome of the flames ſuch a motion and vibration in the air, as if they were become ſo weak in that place, as not to be diſtinguiſh'd. He was moſt of all ſurpris'd, that fire ſhot forth every way out of ſuch a particle of diamond, flaſhing faintly and like lightning at a diſtance; and this he obſerv'd ſeveral times.

M. *Leewenhoeck* did not only himſelf view with his microſcops the ſaid piece of diamond, but he ſhew'd it another perſon, who affirm'd, that the appearances exactly agreed with M. *Leewenhoeck*'s deſcription, and that he was ſurpris'd at the unuſual ſight.

Moreover, he had before his microſcope a piece of diamond whoſe particles were in *lamellæ* or layers on each other; and repreſented by ABC Fig. 1. Plate VII.

Afterwards he placed before his microſcope another piece of diamond, whoſe *lamellæ* he could obſerve very diſtinctly lie on each other; and repreſented by DEFGHIKLM Fig. 2. where the particles mark'd FG, FH, FI, FK and L ſeem to be ſomewhat thicker than the reſt; but theſe conſiſt of ſeveral *lamellæ* lying upon each other. In that part of the

 piece

piece, repreſented between DEFM, exceeding thin *lamellæ* appear very diſtinctly from a congeries of which it is certain the whole diamond is compos'd.

M. *Leewenhoeck* had ſent him two pieces of diamond; as alſo a ſmall diamond, that was cut or poliſhed, but foul or flawy, as jewellers term it.

The ſmall lines running thro' all the piece NOPQ Fig. 3, are really no other than the *lamellæ*, of which diamonds are ſaid to be compos'd; which are more diſtinctly ſeen at PQ.

In order to have a more diſtinct idea of this he caus'd the deſigner to draw it of the ſame bigneſs, as it appear'd to the naked eye; and this is repreſented between ST Fig. 4. This ſmall piece of diamond conſiſted of ſo many and ſuch ſmall particles, as cannot be conceiv'd by any, who have not ſeen it.

In another piece of diamond, almoſt of the ſame bigneſs, the *lamellæ* could be diſtinguiſh'd; and about $\frac{1}{3}$ part thereof conſiſted of ſo regular a *pentagon*, as if it were artificially cut, only that a very ſmall diamond was faſtened to it, that cover'd about the fourth part of the *pentagon*; and he could plainly ſee, that it alſo conſiſted of *lamellæ* or particles reſembling *lamellæ*.

As to the ſmall diamond, tho' poliſhed, it was foul or flawy; ſuch being ſo as are either of a yellowiſh colour or full of lines or chinks, tho' thoſe in this could not be obſerv'd with the naked eye, nay, ſcarcely with the microſcope.

This ſmall diamond was quadrangular; near one of the angles within the diamond, M. *Leewenhoeck* obſerv'd various particles, ſeparated from each other, only that they ſeem'd to touch each other a little, at which he was at firſt ſurpris'd; the largeſt of theſe particles was of a yellowiſh colour, and broader than the other, but not ſo thick, and as bright as glaſs; the other particles were of various figures, and ſome as bright as the firſt; and M. *Leewenhoeck* judged them about 20 in number, tho' the deſigner had not delineated ſo many.

Upon viewing this, M. *Leewenhoeck* was apt to think, that when the matter, of which diamonds are form'd, floated in the air; theſe ſmall particles, which likewiſe were diamonds, were united to the former diamond.

Upon this occaſion M. *Leewenhoeck* recollected, that he had ſeveral mountain chryſtals of an hexangular figure, within ſome of which lay ſome very ſmall oblong figures, of a

a blueiſh colour; but ſo exceeding ſmall, as hardly to be diſtinguiſh'd, even in the beſt microſcope.

Moreover, he caus'd delineate the flawy diamond, as big as the life, as repreſented between V and W Fig. 5.

Afterwards he caus'd delineate ſeparately thoſe ſmall diamonds, included in the larger one, repreſented in Fig. 5. and ſhewn at X Y Z A B C Fig. 6. where X Y Z denotes the outer ſide of the diamond, which tho' cut or poliſhed, yet view'd thro' a microſcope, appear'd of a darkiſh colour; Z A B C D E repreſents thoſe ſmall diamonds, which lay inclos'd, as it were, in the larger diamond.

Afterwards placing before his microſcope that ſide of the diamond, repreſented Fig. 5. and in which the ſmaller diamonds were inclos'd, as has been ſaid, he found it full of holes, which M. *Leewenhoeck* ſuppoſes, were made in poliſhing that ſide: So that the ſmall diamonds, which poſſeſſed thoſe places, falling out in the poliſhing, caus'd theſe pits or holes, repreſented between F G H Fig. 7.

Moreover, turning that ſmall diamond (repreſented between V W Fig. 5.) upon its ſide; and where it was thickeſt, applying a razor, in order to ſplit it with the blow of a hammer; but after repeated blows he could not effect it.

Wherefore, folding the diamond up in a clean piece of paper, he laid it upon a hammer, and after ſtriking ſeveral blows with another hammer, he at laſt broke it. After he had placed all the broken pieces before different microſcopes, he caus'd the deſigner delineate one piece, that ſeemed to the eye to have more *lamellæ* than the reſt, as repreſented by I K L M Fig. 8. Yet it was not poſſible for the deſigner to draw it ſo compleatly as it appear'd to the eye.

The microſcope, with which that piece, repreſented Fig. 8. was obſerv'd, being different from the other microſcopes, by which the other pieces of diamond were delineated; the deſigner drew this laſt piece of the ſame bigneſs, as it appear'd to the naked eye, as repreſented between N O Fig. 9.

Some of the pieces of the diamond, view'd with a microſcope, exhibited a very agreeable ſight; and M. *Leewenhoeck* ſhew'd them to others, who were mightily pleas'd to obſerve ſuch a variety of parts in one ſingle piece of diamond; eſpecially that the *lamellæ*, of which diamonds are compos'd, could

could be very diſtinctly obſerv'd in two ſmall pieces; to wit, when theſe *lamellæ* lay lengthways before the eye.

Afterwards M. *Leewenhoeck* turn'd his thoughts to examine an hexagonal piece of mountain chryſtal, whoſe length was about two fingers breadth, and its thickneſs that of the little finger.

He broke that chryſtal into ſeveral pieces, which he placed before his microſcopes, in order to examine, whether they were compos'd of *lamellæ*, laid upon each other, in the ſame manner as he ſaid, diamonds acquire their bulk: But after repeated trials, he could not find even the leaſt *lamella* therein: But he generally found, in the ſix ſides of all the pieces of chryſtal he had, ſmall tranſverſe lines, ſome a little higher than the others.

Notwithſtanding M. *Leewenhoeck* had formerly conſidered and broke a great many chryſtals, yet he could never ſatisfy himſelf in this matter.

The different Refrangibility of coloured Light; by Dr. Deſaguliers. Phil. Tranſ. N° 374. p. 206.

SIR *Iſaac Newton* in his Optics, *book* 1. *prop*. 1. *exper.* 2. gives an account of an experiment, made with a card or paper, painted red on one half and blue on the other; which being enlightened by a candle, the image, by the interpoſition of a *lens*, is ſo projected on a white paper, held on the other ſide of the *lens*, that the place where the blue half appears diſtinct, (or the diſtinct baſe of the image of the blue half, as opticians call it) is much nearer to the *lens* than the place of the image of the red half; and this is made apparent, by ſeeing on one of theſe images the repreſentation of the black threads, wrapp'd round the card, whilſt they are not viſible on the other: This is fully deſcrib'd in *loc. ſupra citat.* But yet a foreign gentleman (vide *Acta Erudit. Lips. ſupplem. tom.* 8. § 3. *p.* 130, 131.) has call'd the experiment in queſtion, and denied the matter of fact, affirming, that he could not make it ſucceed; but propoſes an experiment of his own to diſprove the different refrangibility of the rays,

Upon this Dr. *Deſaguliers* made the experiment over again before the Royal Society, which ſucceeded well: But becauſe there muſt be care taken in making it, he mentions all the particulars, obſerv'd in the performance; which, if duly

duly put in practice, will make the experiment always succeed.

He painted one half of the card R B (in Fig. 10. Plate VII.) as B with *ultramarine*, made deeper with a small mixture of indigo; and the other half R he painted over with *cinnaber*, heighten'd with a little *carmine*; so that the line, which separated the red from the blue, was perpendicular to the long sides of the card.

Then he wrapp'd a black silk four times over the middle of each painted part of the card, as in Fig. 11.

Upon a square trencher (as in Fig. 12.) painted black, and suspended vertically against a wall, the Dr. fix'd the card with a pin; and the room being made very dark, he enlightened the card with a strong light, thrown upon it from a dark lanthorn, that had two convex glasses therein; then setting up the *lens* L L (represented by Fig. 13.) in such a manner that its axis pass'd perpendicularly thro' the image of the card, at the distance of nine foot from the card, the image of the card being receiv'd upon a piece of white paper at the distance of nine foot on the other side of the *lens* at B, the blue half appeared distinct, with the image of the black silk going vertically along its plane, whilst no appearance of the black silk was perceivable on the red half; then removing the paper about two inches to R, the red half of the image had a black line very plain upon it, whilst it was invisible on the blue half. This was more evident, when a strong image of the candle was successively thrown on that half of the card, whose image was then under consideration: When the paper was held in the middle between R and B, the black line upon each colour was visible, but indistinct.

N. B. Care must be taken to make the colours deep, because having accidentally rubb'd off some of the blue, the whiteness of the card under it, caus'd its image to fly out farther, almost as far as that of the red.

An Account of the Inoculation of the Small-Pox *and the Mortality of that Distemper in the natural Way; by Dr.* Nettleton. Phil. Transf. N° 374. p. 209.

THERE are two propositions, advanced by the favourers of the practice of inoculation, in which the public seems to require fuller satisfaction. 1. That the distemper, rais'd by inoculation, is really the small-pox. 2. That it is

is much more mild and favourable, and far leſs mortal than the natural ſort.

The former of theſe is not ſo much diſputed now, as it was at firſt, when this method was introduced; nor can it be doubted by any one, who has ſeen thoſe that have been inoculated, and has alſo been much converſant in the natural ſmall pox: There is uſually no manner of difference to be obſerv'd between the one ſort and the other, when the number of puſtules is nearly the ſame; but in both there are almoſt infinite degrees of the diſtemper, according to the diverſity of that number. All the variation, that can be perceiv'd in the inoculated ſmall-pox from the natural, is, that in the former, the puſtules are commonly fewer in number, and all the reſt of the ſymptoms in the ſame proportion more favourable: They exactly reſemble what is call'd the diſtinct ſort; the ſymptoms before the eruption are the very ſame, and when the puſtules begin to riſe, their appearance is the ſame, as well as their periods of maturation and declenſion; they are at firſt of the ſame florid, roſy colour, and when fully ripe, of as fair a yellow; they commonly riſe as round and as large as the other, and when they are very numerous, the inflammation and ſwelling of the face comes on at the uſual time, and is followed by the ſwelling of the hands and feet, and only once the Dr. obſerv'd a ſalivation, tho' the puſtules were diſtinct: In the natural ſmall-pox, when the puſtules are very few, we ſometimes obſerve, they do not riſe to ſo great a bulk, neither do they ripen ſo fully nor continue ſo long as uſual; and it is the ſame in the way of inoculation: In ſhort, as this diſtemper is rais'd by an ingraftment from the ſmall pox, as it has the very ſame appearance, and as it is capable of producing the ſame by infection, there ſeems to be no room to doubt of its being the true and genuine ſmall-pox: And if that be allow'd, it will follow from thence, as a corollary, *viz.* That thoſe, who have been inoculated, are in no more danger of receiving the diſtemper again, than thoſe who have had it in the ordinary way: And this is alſo thus far confirm'd by experience. It is very readily granted, that the operation may ſometimes fail: Thoſe gentlemen, who firſt communicated to the Royal Society ſome account of this practice from *Turkey*, intimated ſo much; tho' Dr. *Nettleton* believes that that will happen but rarely: In one inſtance in *Hallifax*, vide *Phil. Tranſ.* N° 370, the Dr. obſerv'd no eruption at all, nor did the

[th]e wound inflame and ſwell any more than in a common inci-[si]on, which made him conclude, that what was applied had [no]t taken effect, and indeed, he was well aware of the reaſon. [I]n three other inſtances, tho' the wounds inflamed, ſwelled and [d]iſcharged conſiderably; yet the eruptions were ſo imperfect, as [to] leave the Dr. a little in doubt: But two of theſe were after-[w]ards ſufficiently tried, by being conſtantly with thoſe who had [th]e ſmall-pox, without receiving any infection; which makes [th]e Dr. apt to think, they will always be ſecure from any dan-[ge]r of it: As to all the reſt, neither the Dr. nor any body elſe, [w]ho ſaw them, did in the leaſt queſtion, but that they had the [tr]ue ſmall-pox.

As to the latter propoſition, *viz.* That the inoculated ſmall-[p]ox is far leſs dangerous than the natural, the truth hereof, the [D]r. ſuppoſes, can only be found by making a compariſon, ſo [fa]r as experience will extend. To this purpoſe he took an ac-[co]unt how many had the ſmall-pox and how many out of that [nu]mber died in the town of *Halifax* and ſome part of the [co]untry, and he procured the ſame from ſeveral other towns [th]ereabouts, where the ſmall-pox had been epidemical in 1722, [an]d that with as much exactneſs as was poſſible.

	Perſons who had the ſmall-pox.	died
[I]n *Halifax*	276	43
[I]n a part of the pariſh at *Halifax* ſtretching towards *Bradford*.	297	59
[I]n another part of the ſame pariſh	268	28
[I]n *Bradford*	129	36
[I]n *Leeds*	792	189
[I]n *Wakefield*	418	57
[I]n *Rochdale*	177	38
[I]n *Aſhton under Line*, a ſmall market-town in *Lancaſhire*, including two neighbouring villages.	279	56
[I]n *Macclesfield*	302	37
[I]n *Stockport*	287	73
[I]n *Hatherfield*	180	20
Total	3405	636

A great number of obſervations is requiſite, before any cer-[ta]in concluſions can be drawn. The Dr. only remarks, that it

appears

appears from these accounts, that in 1722, in this part of th kingdom, almost 19 out of every 100, or near one fifth o those, who had the natural small-pox, died; whereas out of 61 inoculated, not one died.

A Comparison between the Danger of the Small-pox *in th Natural Way and that given by Inoculation; by Dr.* Jurin Phil. Trans. N° 374. p. 213.

THE number of persons, who had the small-pox by inocu lation in *England*, is, by the best information the D could procure, as follows;

Inoculated by Dr. *Nettleton* 6
Mr. *Claudius Amyand* serjeant-surgeon 1
Mr. *Maitland* surgeon 5
Dr. *Dover*
Mr. *Weymish* surgeon
The Rev. Mr. *Johnson*
Dr. *Brady* at *Portsmouth*
Mr. *Smith* surgeon and Mr. *Dymer* apothecary at *Chichester* 1
Mr. *Waller* apothecary at *Gosport*
A woman at *Leicester*
Dr. *Williams* at *Haverford-West*
Two other persons near the same place
Dr. *French* at *Bristol*

In all 18

The opposers of inoculation affirm, that out of this numb two persons died of the inoculated small-pox; and the favoure of this practice maintain, that their death was owing to oth causes. If, to avoid dispute, these two be allowed to have die of inoculation, we must estimate the hazard of dying of th inoculated small-pox, as far as can be collected from our ow experience, to be that of 2 out of 182 or 1 out of 91.

Mr. *Mather*, in a letter, dated *March* 10. 1721, from *Bost* in *New England*, gives an account, that of near 300 inoc lated there, five or six died upon or after it; but from other d seases and accidents, chiefly from having taken the infection i the common way by inspiration, before it could be commun cated to them in this way of transplantation.

If we allow 5 out of these 300 to have died of the smal pox by inoculation, notwithstanding what Mr. *Mather* h said of their dying by other accidents or diseases; the hazar

inoculation will thence be determined to be that of 1 in bout 60: But here it must be observed, that by all the accounts om *New England*, the operators there do not appear to have een so cautious in the choice of their subjects, as in *England*: or, Mr. *Mather* tells us, that the persons inoculated were oung and old, from 1 to 70 years of age, weak and strong; nd by other relations we are informed, that women with child, nd others even in child bed, underwent the operation: Probaly the greatness of the danger they were in, from the infection the natural way, which then raged among them with the utost fury, made them the more adventurous.

In order to form an estimate of the hazard, which all manind, one with another, are under of dying of the small-pox the natural way, that by comparing this with the hazard of oculation, the public may be enabled to form a judgment, hether or no the practice of inoculation tend to the preservaon of mankind, by lessening the danger to which they are therways liable; the Dr. consulted the yearly bills of mortaity, as far back as 1667, the year after the plague and the fire f *London*, comprehending to 1722, the space of 56 years, om 42 of which the Dr. has given extracts in the two followng tables.

The first of these takes in the first 20 years, distinguishing or every year the total number of burials, and likewise the umber that died of the small-pox in two separate columns. The third column shews, how many died of the small-pox out of every 1000 that were buried; and the fourth column exhibits the proportion between those that died of the small-pox and he whole number of burials, by the nearest vulgar fraction, having always 1 for the numerator.

The second table gives the last 22 years after the same manner; and at the bottom of each table is given the total number for each series of years, and likewise the number that died each year, taken at a medium, one year with another: By which it ppears, that the proportion between the number of those that die of the small-pox and the whole number of burials, is very nearly the same, upon an average for each series of years.

The 14 intermediate years between 1686 and 1701 are left out; because in the bills for those years, the accounts of the small-pox and measles are not distinguished, as in the preceeding and following years; but are joined together in one article: So that from them no certain account can be drawn of the number of persons that died of the small-pox.

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Dr. *Brady* at *Portsmouth*
Mr. *Smith* surgeon and Mr. *Dymer* apothecary at *Chichester*
Mr. *Waller* apothecary at *Gosport*
A woman at *Leicester*
Dr. *Williams* at *Haverford-West*
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inoculation will thence be determined to be that of 1 in [ab]out 60: But here it muſt be obſerved, that by all the accounts [fr]om *New England*, the operators there do not appear to have [b]een ſo cautious in the choice of their ſubjects, as in *England*: [F]or, Mr. *Mather* tells us, that the perſons inoculated were [y]oung and old, from 1 to 70 years of age, weak and ſtrong; [an]d by other relations we are informed, that women with child, [an]d others even in child bed, underwent the operation: Proba[b]ly the greatneſs of the danger they were in, from the infection [in] the natural way, which then raged among them with the ut[m]oſt fury, made them the more adventurous.

In order to form an eſtimate of the hazard, which all man[k]ind, one with another, are under of dying of the ſmall-pox [in] the natural way, that by comparing this with the hazard of [in]oculation, the public may be enabled to form a judgment, [w]hether or no the practice of inoculation tend to the preſerva[ti]on of mankind, by leſſening the danger to which they are [ot]herways liable; the Dr. conſulted the yearly bills of morta[lit]y, as far back as 1667, the year after the plague and the fire [o]f *London*, comprehending to 1722, the ſpace of 56 years, [fr]om 42 of which the Dr. has given extracts in the two follow[in]g tables.

The firſt of theſe takes in the firſt 20 years, diſtinguiſhing [f]or every year the total number of burials, and likewiſe the [n]umber that died of the ſmall-pox in two ſeparate columns. The third column ſhews, how many died of the ſmall-pox out [o]f every 1000 that were buried; and the fourth column exhi[b]its the proportion between thoſe that died of the ſmall-pox and [t]he whole number of burials, by the neareſt vulgar fraction, [h]aving always 1 for the numerator.

The ſecond table gives the laſt 22 years after the ſame man[n]er; and at the bottom of each table is given the total number [f]or each ſeries of years, and likewiſe the number that died each [y]ear, taken at a medium, one year with another: By which it [a]ppears, that the proportion between the number of thoſe that [d]ie of the ſmall-pox and the whole number of burials, is very [n]early the ſame, upon an average for each ſeries of years.

The 14 intermediate years between 1686 and 1701 are left [o]ut; becauſe in the bills for thoſe years, the accounts of the [ſ]mall-pox and meaſles are not diſtinguiſhed, as in the preceed[in]g and following years; but are joined together in one article: So that from them no certain account can be drawn of the num[b]er of perſons that died of the ſmall-pox.

TABLE I.

Years	Total N° of Burials	Died of the Small Pox. In all	In 1000	In Propor.
1667	15842	1196	75	1/33
1668	17278	1987	115	1/9
1669	19432	951	49	1/20
1670	20198	1465	73	1/14
1671	15729	696	44	1/23
1672	18230	1116	61	1/16
1673	17504	853	49	1/21
1674	21201	2507	118	1/8
1675	17244	997	58	1/17
1676	18732	359	19	1/52
1677	19067	1678	88	1/11
1678	20678	1798	87	1/12
1679	21730	1967	91	1/11
1680	21053	689	33	1/31
1681	23971	2982	125	1/8
1682	20691	1408	68	1/15
1683	20587	2096	102	1/10
1684	23202	156	7	1/149
1685	23222	2496	107	1/9
1686	22609	1062	47	1/21
20 Years	398200	28459	71½	1/14
Each Y. at a *Medium*	19910	1423	71½	1/14

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TABLE II.

Years	Total N° of Burials.	Died of the Small Pox. In all	In 1000	In Propor.
1701	20471	1095	53	$\frac{1}{19}$
1702	19481	311	16	$\frac{1}{63}$
1703	20720	898	43	$\frac{1}{22}$
1704	22684	1501	66	$\frac{1}{15}$
1705	22097	1095	50	$\frac{1}{20}$
1706	19847	721	36	$\frac{1}{28}$
1707	21600	1078	50	$\frac{1}{20}$
1708	21291	1687	79	$\frac{1}{13}$
1709	21800	1024	47	$\frac{1}{21}$
1710	24620	3138	127	$\frac{1}{8}$
1711	19833	915	46	$\frac{1}{22}$
1712	21198	1943	92	$\frac{1}{11}$
1713	21057	1614	77	$\frac{1}{13}$
1714	26569	2810	106	$\frac{1}{9}$
1715	22232	1057	48	$\frac{1}{21}$
1716	24436	2427	99	$\frac{1}{10}$
1717	23446	2211	94	$\frac{1}{11}$
1718	26523	1884	71	$\frac{1}{14}$
1719	28347	3229	114	$\frac{1}{9}$
1720	25454	1440	57	$\frac{1}{18}$
1721	26142	2375	91	$\frac{1}{11}$
1722	25750	2167	84	$\frac{1}{12}$
22 Years	505598	36620	72	$\frac{1}{14}$

Each

	Tot. N° Bur.	Died of the Small Pox. In all	in 1000	in Pro.
Each Year at a *Medium.*	22982	1665	72	$\frac{1}{14}$
42 Ye.	903798	65079	72	$\frac{1}{14}$
Each Year in 42 at a *Medium*	21519	1550	72	$\frac{1}{14}$

By those tables it appears, that upwards of 7 *per cent* or somewhat more than $\frac{1}{14}$ part of mankind die of the small pox; and consequently, the hazard of dying of that distemper, to every individual, born into the world, is at least that of 1 in 14; and that this hazard increases, after the birth, as the child advances in age, will appear from what follows.

From this estimate it is demonstrable, that in the case of persons actually having the small-pox, the hazard they run, one with another, of dying of that distemper, is greater than that of 1 in 14; or which is the same thing, there must be fewer than 13 that recover, for one that dies of the small-pox: For, since $\frac{1}{14}$ part of mankind die of the small-pox and the other 13 parts die of other diseases; if these 13 have all had the small-pox and recovered from them, before they fell ill of those other diseases of which they died, then just 13 will have recovered from the small-pox, for one that dies of that distemper: But as it is notorious, that great numbers, especially of young children, die of other diseases, without ever having the small-pox; it is plain, that fewer than 13 must recover from this distemper, for one that dies of them.

To determine exactly how many of these 13 parts of mankind die, without having the small-pox, is a very difficult task: But it is easy to see, that a considerable deduction is to be made from them.

In the first place, the two articles of still-born and abortive children, which are put into the yearly bills, as part of the number of burials, are unquestionably to be deducted.

With these two the Dr. joins the following heads, which, by the best information he could procure, comprehend only very young children, or at most not above one or two years of age; as overlaid, chrysoms and infants, convulsions, horseshoehead, headmoldshot, teeth, water in the head, worms, rickets, liver-grown,

grown, chin-cough and hooping-cough; which articles in the yearly bills for 22 years laſt paſt, amount at a medium to 386 in each 1000, of the whole number of burials.

It is true, that in all probability, ſome ſmall part of theſe muſt have gone thro' the ſmall-pox; and therefore ought not to be deducted out of the account: But then, on the other hand, as it is certain, that of the remaining $\frac{614}{1000}$ of mankind, that are above one or two years of age, there are great numbers, that never have the ſmall-pox; it will be judged no unequal ſuppoſition, if we ſuppoſe all contained under the above-mention'd heads, to have eſcaped that diſtemper, when by way of compenſation all the reſt of mankind are allowed to undergo them, which conceſſion is ſo large, that it will abundantly make up for what is aſſumed too much in the former ſuppoſition.

Allowing, therefore, that out of every 1000 children, that are born, 386 die under 1 or 2 years of age, without having the ſmall-pox, and 72 do ſome time or other die of that diſtemper; it follows, that the hazard of dying of them to the remainder of mankind, above 1 or 2 years of age, who are all ſuppoſed to undergo that diſeaſe ſooner or later, is that of 72 out of 614, or nearly 2 out of 17: So that no more than between 7 and 8 can recover from that diſtemper, for one that dies of them: And if any conſiderable part of the aforeſaid remainder of mankind, more than is allowed for above, eſcape having the ſmall-pox, then the proportion of thoſe that recover from them, will be ſtill ſmaller.

This conſideration ſhews the fallacy of one plauſible argument, that has been often made uſe of on occaſion of the diſputes about inoculation; *viz.* that whatever be the danger of dying of the ſmall-pox, to thoſe that actually have that diſeaſe; yet as great numbers of perſons never have the ſmall-pox at all, this danger is what any particular perſon may never be in, and therefore, it will be madneſs to undergo the hazard of inoculation, be it great or ſmall, in order to prevent a diſeaſe, which poſſibly may never befal one.

For, if 2 parts in 17 of all mankind, that are above one or two years of age, muſt ſooner or later die of the ſmall-pox; it is plain, that how many parts ſoever of theſe 17 are ſuppoſed to eſcape that diſtemper, the mortality among the remainder who undergo them, muſt in proportion be ſo much the greater. As for inſtance, if 7 parts eſcape having the ſmall-pox and 10 under-

undergo them, then 2 out of 10, or 1 out of 5, that have the small-pox, must die of them.

And as it can never be known, whether or no any particular person be one of those, that are to have the small-pox; his hazard of dying of that distemper being made up of the hazard of having it, and the hazard of dying of it; if he have it, will be exactly the same, namely, that of 2 in 17, or 1 in 8 or 9, whether the proportion of mankind, that escape having the small-pox, be great or small.

But as what has been said concerning the hazard of the small-pox in the natural way, is taken from an account of 42 years: Whereas, the hazard of inoculation is estimated only from what has happened in the space of about 18 months, since which time it had its first rise among us; it will, perhaps, be asked by some persons, why the estimate of the hazard of the natural small-pox is not also made, from the last two years alone, without running back into so great a number of years, before inoculation was begun?

To which the Dr. answers, that the proportion of such, as die of the small-pox, varies so much in different years, as appears from the tables above, that it was impossible to come at any certainty in this point, from the consideration of the two last years alone; and if any one suspect the Dr. of partiality in proceeding in the manner he has done, he need only cast his eyes upon the second table, where he will find, that the mortality of the natural small-pox, for the 2 last years, has considerably exceeded the medium he has determined, from taking in 42 years.

There is another method, which, if it were put in practice in several large towns or parishes, and for a sufficient number of years, would enable us to come at a nearer and still more certain computation of the proportion between those that recover, and those that die of the small-pox; which is, to send a careful person once a year, from house to house, to enquire what persons have had the small-pox, and how many have died of them in the preceeding year. This was done in some places, as follows.

Sick

	Sick of the small-pox	died
...veral towns in *Yorkshire*	3405	636
...*hichester*	994	168
...*averford-West*	227	52
Total	4626	856

From which it appears, that upon a medium between these ...counts, there died of the small-pox almost 19 *per cent*, or ...arly 1 in 5, of persons of all ages, that underwent that dis...mper. Which is the more to be remarked; for, that out of ... persons, that had the small-pox by inoculation, the same ...ar, and in the neighbourhood of the same places, not one ...iscarried.

Mr. *Mather* observes, in his abovementioned letter, that out ... more than 5000 persons that had the small-pox at *Boston* in ...*ew-England*, within little more than half a year, near 900 ...ed, which is more than 1 in 6; and this account added to ...ose from *Yorkshire*, *Chichester* and *Wales*, reduces the pro...ortion of those that die of the small-pox to somewhat more ...an 18 *per cent*; so that the hazard of dying of that distem...r, to those who are taken ill of it, is that of 1 in between ... and 6, or something above 2 in 11.

The result, therefore, of these computations is, that if the ...me proportions should still continue, as have hitherto been ...termined by observation, we must expect.

That of all the children that are born, there will, some time ...other, die of the small-pox, 1 in 14.

That of persons of all ages taken ill of the natural small...ox, there will die of that distemper, 1 in 5 or 6, or 2 in 11.

That of persons of all ages, inoculated, without regard to ...e healthiness or unhealthiness of the subject, as was practised ... *New-England*, there will die 1 in 60.

That of persons, inoculated with the same caution in the ...oice of the subjects, as was used by the several operators one ...ith another, in *England* (if we allow in the two disputed ...ses abovementioned, that the patients died of the inoculated ...all-pox) there will die 1 in 91.

But if those two patients be allowed to have died of other ...cidents or diseases; then we shall have reason to think, as far ... any judgment can be made from our own experience in

England, that none at all will die of inoculation, provided that proper caution be uſed as in *Turkey*, where out of many thouſands, that in the ſpace of 40 years were inoculated in and about *Conſtantinople*, by one *Greek* woman, not ſo much as one perſon miſcarried, as the Dr. was aſſured by the ingenious Dr. *le Duc*, a native of *Conſtantinople*, who was himſelf inoculated there, under the care of his father, an eminent phyſician in that city.

The following account of the ſucceſs of inoculation in and about *Boſton* in *New England*, was procured from Capt. *Osborne*, who reſided in that town and neighbourhood, during the whole time of that practice.

The Dr. inſerts it here, as it confirms the extract given above from Mr. *Mather*'s relation, and is a more particular account of the matter of fact, than any he had hitherto ſeen.

In *May* 1721, the ſmall-pox were brought into the town of *Boſton*; in *June* they began to ſpread pretty much, and in *July* they were got into moſt parts of the town and a conſiderable number of people died of them: At this time inoculation was firſt put in practice by Dr. *Boyleſton*, who then performed it upon his own child, and a *Negro* ſervant, who both did well; notwithſtanding which, this attempt gave great uneaſineſs to the neighbours: However the practice went on, to the number of about 40 perſons, one of which was a woman of about 40 or 45 years of age, who got well over the ſmall-pox, as her husband publickly declared, but had been before troubled with hyſteric fits, of which ſhe died ſome little time after: When about 70 perſons had undergone the operation, Capt. *Osborne* and his wife, who had hitherto been at a place called *Roxbury*, a mile from *Boſton*, went into town and received the ſmall-pox by inoculation. They had them with all the gentleneſs and moderation that was poſſible, neither of them having had an hundred puſtules, or being ſenſible of any fever worth mentioning; ſo that they did not find it neceſſary to keep their beds.

In *Auguſt* the natural ſmall-pox proved more mortal, and inoculation made a greater progreſs, the people continuing to come into the practice of it: A ſecond perſon, that died after inoculation, was an apothecary's houſekeeper, that was out of town, till an *Indian* maid got the diſtemper in the ſame houſe and removed and died. Upon which this woman coming to town, her maſter undertook to perform the operation upon her (which by the bye was the firſt and laſt he ever performed) and on the third day after the inoculation, the ſmall-pox came out upon

pon her very full: From which it was plain, that ſhe had aken the infection before, in the common way.

The third perſon, that died after inoculation, was a Gentleman of 45 years of age, that lodged in the ſame houſe with he Capt. and his wife; he was under great and extreme infirmity of body, and upon making the experiment he had not trength to go thro' with it.

The ſaid Gentleman's ſiſter was the fourth perſon that died upon this operation: She was about 40 years of age, of great ndiſpoſition of body, and weak, as was her brother.

The fifth, that died upon inoculation, was a woman ſervant a houſe, where the whole family, to the number of 8, were noculated at the ſame time: She lay in a cold upper room luring her illneſs and was much neglected, the whole family eing down together; ſo that ſhe died merely for want of a ittle attendance. This happened in the town of *Roxbury*, where it is to be obſerved, that 13 men, maſters of families, got the ſmall-pox and all died, which induced the people to make uſe of inoculation, having been before much againſt it, nd there were 43 men inoculated there, who all did well. The miniſter of the town was the firſt who put it in practice there, much againſt the mind of the people at firſt; tho' afterwards hey were well pleaſed with it; ſeeing with what great ſucceſs it was attended; and then whole families came into it and underwent the operation. There were in all at leaſt 280 perſons inoculated; and the Capt. ſuppoſes there might have been about 20 or 30 more, of which he could give no certain account.

An Account of a new Sort of Moloſſes, *made of* Apples, *and of the degenerating of* Smelts; *by Mr.* Dudley. Phil. Tranſ. N° 374. p. 231.

THE apple, that produces the moloſſes, is a ſummer-ſweeting, of a middling ſize, pleaſant to the taſte and full of juice, ſo that ſeven buſhels will make a barrel of cyder. The manner of making it is thus; you grind and preſs the apples, and then take the juice and boil it in a copper, till $\frac{3}{4}$ of it is waſted, which will be done in about ſix hours gentle boiling; and by that time it comes to be of the ſweetneſs and conſiſtency of moloſſes.

Some people ſcum the cyder as it boils, others do not, and yet there ſeems to be no great difference in the goodneſs.

These new moloſſes anſwer all the ends of that made of the ſweet cane, imported from beyond ſea. They ſerve not only for food and brewing, but are alſo of great uſe in preſerving of cyder; two quarts of them put into a barrel of rack'd cyder will both preſerve and give it a very agreeable colour.

The apple moloſſes were accidentally diſcovered a few years ago by Mr. *Chandler* at *Woodſtock* in *New England*, a town remote from the ſea, and where the *Weſt India* moloſſes are dear and ſcarce: He ever after ſupplied his family with moloſſes out of his orchard, and his neighbours likewiſe did the ſame to their great advantage.

The country farmers run much upon planting orchards of theſe ſweetings, for fatning their ſwine, and they aſſured Mr. *Dudley*, they make the beſt ſort of pork: And he affirms that the cyder made of them is better than that of other fruit for taſte, colour and keeping.

Two ſhort miles from Mr. *Dudley*'s houſe there was a fine pond, half a mile over, having little or no communication with the ſea: An ingenious man, ſome threeſcore years ago, for experiment ſake, took a pail of large ſmelts from the river and put them into this pond, where they multiplied abundantly; but they degenerated to a very ſmall ſort; for, the river ſmelts Mr. *Dudley* ſuppoſes are full as large as thoſe of the *Thames*, ſome of them weighing two ounces and $\frac{1}{2}$; whereas theſe ſmall ones did not weigh five penny weight. The pond-ſmelts are thought to eat much better than the other, and they are very tranſparent and of a beautiful ſhining pearl-colour.

Obſervations on the Eclipſe *of the* Moon, June 18, 1722; *and the Longitude of* Port Royal *in* Jamaica, *determined thereby*; *by Dr.* Halley. Phil. Tranſ. N° 375. p. 235.

THE eclipſe of the moon, that happened in *June* 1722 was ſo far hid by the cloudy ſky, that neither Dr. *Halley* nor any other in or about *London*, could furniſh an obſervation thereof, worthy to be laid before the *Society*. But the ſame having been well obſerved at *Jamaica* by Capt. *Candler*, commander of his Majeſty's ſhip the *Launceſton*, and at *Berlin* by M. *Chriſtfried Kirch*, aſtronomer of the *Royal Academy* of *Sciences* there; the Dr. prefixes to their accounts that little he himſelf was able to note concerning it.

June 18. in the morning, having perfectly rectified his clock ſo as to ſhew the apparent time; neither the tranſit of the moon over

over the meridian, nor the beginning of the eclipſe, which ſoon follow'd, could be obſerv'd by reaſon of a very thick cloud.

At 13h 12' app. time a ſmall portion of the moon's body was ſeen thro' a very little *hiatus* in the cloud, by which glimpſe the Dr. could only be aſſur'd, that the eclipſe was not yet total.

At 13h. 22' by ſuch another view he was ſatisfied, that it was now become total; but in a moment it again diſappear'd, till 14h. 49' 10", when the cloud beginning to break, he got time to meaſure with the micrometer the lucid parts, now recover'd in the moon's diameter, which he found 14', tho' not ſo well as he could wiſh, by reaſon of a thinner ſort of cloud, which perpetually interpos'd, and render'd the edge of the ſhadow ſomewhat dubious.

At 15h. 15' the moon was pretty well got out of the thick cloud; but being very low and the day-light becoming ſtrong, ſhe ſhone very faintly and the ſhadow became worſe and worſe defin'd.

From 15h. 26' to 15h. 27' T. app. the Dr. doubted of the end, and he is poſitive it did not exceed the 27'. It ended over againſt the north part of *Palus Mæotis Hevelii*, much about the middle of the weſtern or right hand limb of the moon, ſhe being then very near ſetting.

Capt. *Candler*, being at that time at *Port Royal* in *Jamaica*, had much better fortune and a ſerene ſky from the beginning to the end; and having us'd due care to be aſſur'd of the times by altitudes, taken with an inſtrument of three foot radius, the reſult of his obſervations is, as follows.

	h	'	"
The eclipſe began	6	59	10
Immerſion	8	7	50
Emerſion	9	11	00
The end	10	19	40
Whence the middle	8	39	25

And ſuppoſing the eclipſe to have ended at *Greenwich* at 15h 26' and ½; the difference of Long. between *Port Royal* and *Greenwich* will be 5h 6' 50" or 5h 6' and ½ from *London*, that is 76° 37' and ½.

M. *Kirch*, being in a more eaſterly meridian, could obſerve nothing of the emerſion, but he carefully noted the time

time of the beginning and immersion, as he observ'd them at *Berlin*; viz. the beginning of the eclipse at 12h 59′ 55″, and the immersion at 14h 8′ 8″. Now by comparing several observations, made at both places, the Dr. formerly concluded *Berlin* to be 54′ of time, or 13° and ½ more easterly than *London*: Wherefore, at *London* it began at 12h. 5′ 55″ and immerged at 13h. 14′ 8″; that is, the beginning was later there than at *Jamaica* 5h. 6′ 45″, and the emersion later 5h. 6′ 18″, punctually agreeing with what resulted from the Dr's observation of the end, as aforesaid, and sufficiently with what he had long since determined from observations sent him from *Jamaica* by Mr. *Charles Boucher*.

The Longitude of Carthagena *in* America; *by the Same.* Phil Transf. N° 375. p. 237.

AMONGST several observations, sent from *Carthagena* in *America*, and made by Colonel *Don Juan de Herrera*, chief Engineer of that city, Dr. *Halley* found one immersion of the first satellite of *Jupiter* into his shadow, observ'd there by a 17 and ½ foot telescope on *April* 9. 1712, O. S. 15h 58′ 44″ apparent time; as also two emersions of the same satellite, on *July* 5, 11h 23′ 41″ and *July* 21, 9h 42′ 17″ 1723, O. S. all which tally with observations, made at *Wansted* by Dr. *Pound* and Mr. *Bradley*, who observ'd there the very next eclipses to all the three; that is, the immersion by a 15 foot telescope on *April* 11. 15h 28′ 40″ equat. time, or 15h 30′ 25″ app. time; and the first emersion *July* 7. 10h 59′ 28″ equat. time by the reflecter, and 18″ after, or 10h 59′ 46″ by the 15 foot telescope, that is, 10h 54′ 12″ app. time. The other was observ'd at *Wansted*, *July* 23, 9h 19 10″ equat. time, both by the reflecter and 15 foot telescope; that is, at 9h 13′ 35″ app. time. Substract from each of these one period of this satellite or 1° 18h 28′ 36″, and *April* 9, 15h 58′ 44″ at *Carthagena* will be 21h 1′ 49″ of the same day at *Wansted*, and the difference of meridians 5h 3′ 5″; likewise by the first emersion *July* 5, 11h 23′ 41″ at *Carthagena*, was at *Wansted* 16h 25′ 36″ of that day; whence the difference of meridians 5h 1′ 55″. But by the last emersion *July* 21, 9h 42′ 17″ at *Carthagena* was 14h 44′ 59″ at *Wansted*; whence *Wansted* is 5h 2′ 42″ more easterly than *Carthagena*; and taking the medium of all three, 5h 2′ 34″ or 75° 38′ may be taken for the true difference of Long. that is 75° and ½ from *London*, which compar'd with Capt.

Candler's observation, shews *Carthagena* to be about 20 leagues to the eastward of *Port Royal* in *Jamaica*.

Observations on a Comet *seen at* Berlin, *from the* 18th *of* January *to the* 5. *of* February 1718, N. S. *By* Christfried Kirch. Phil. Transf. N° 375. p. 238. *Translated from the* Latin.

HERE M. *Kirch* hints, that the observations of this comet, published in *Nov. Liter. Lipsien.* are not accurate; for, on the 23. of *Januury* in the morning, the comet form'd an isosceles triangle with θ and φ of *Cassiopeia* and not with δ and φ; and in the evening, the φ of *Perseus*, the comet and the θ of *Cassiopeia* were as to sense in a streight line.

M. *Kirch* observ'd the comet from the 18th of *January* to the 5th of *February*: The following table exhibits its places by observations till 10 in the evening.

	Longitude.			Latitude.		
	°	′		°	′	
18 *Jan.*	27	26	*Cancer*	69	18	S.
21 *Jan.*	16	25 ½	*Taur.*	48	42	S.
23 *Jan.*	9	28 ½	*Taur.*	39	45	S.
26 *Jan.*	5	25 ½	*Taur.*	32	55	S.
27 *Jan.*	4	41	*Taur.*	31	24	S.
28 *Jan.*	4	4	*Taur.*	30	13	S.
30 *Jan.*	3	4	*Taur.*	28	23 ½	S.
31 *Jan.*	2	43	*Taur.*	27	40	S.
1 *Feb.*	2	25	*Taur.*	27	1	S.
2 *Feb.*	2	10	*Taur.*	26	22	S.
5 *Feb.*	1	39	*Taur.*	24	53	S.

The

The path of the comet paſs'd above the back of *Urſa Minor*, near the pole ſtar, thro' the legs and knees of *Cepheus*, *Caſſiopeia* and *Andromeda*; its deſcending node was in 21° and ½ of *Aries*, with ſame mutation; the angle of the cometic orbit and ecliptic was about 69° and ½, with ſome variation too; the path of the comet was almoſt 2° from the pole of the world and interſected the equator in 20° and ½ from the equinoctial point; its *perigæum* was in 6° 6' of *Virgo* with 62° 7' N. Lat. The comet was in its *perigæum* on the 18th of *January* 3h 9' in the morning; its diurnal motion in its orbit was 22° 8' in the *perigæum*, *viz.* 12 hours before and 12 hours after the *perigæum*; but on the laſt days of its appearing 32'. Suppoſing the earth at reſt and the comet moving in a right line, the motion of the comet was 391 ſuch parts, whereof the leaſt diſtance of the comet from the earth is 1000. M. *Kirch* could determine nothing with certainty about the parallax of the comet, only that it was a great deal higher than the moon; and he conjectures with ſome probability that it mov'd within the orbits of the planets; nay, that in its *perigæum* it was much nearer to us than the ſphere of *Mars*. For, ſuppoſe the ſemidiameter of the earth's orbit be 10,000 parts, the diurnal motion of *Mars* will be 139 or 140 ſuch parts: If we ſuppoſe the comet to have been in the orbit of *Mars* with 62° 7' Lat. and 22° 8' diurnal motion, its velocity would be 2847 parts, ſuppoſing at the ſame time it was in oppoſition to the ſun; but ſince the difference of Long. of the ſun and comet in its *perigæum* was only 141° 40', the diurnal motion of the comet becomes 3200 parts, and the proportion of the motion of the comet to the motion of *Mars* as 23 to 1: Wherefore, he collects, that the comet mov'd within the ſphere of *Mars*. But ſhould any one ſuppoſe the comet to have moved within the orbit of *Saturn* it ſhould then have a velocity, that would be to the velocity of *Saturn* as 600 to 1, and that in one day it would run over a greater ſpace, than the earth does in half a year; not to mention the diameter of the comet, that ſhould be no leſs than three diameters of the ſun.

M. *Kirch* comparing this comet with others, found that obſerv'd by *Regiomontanus* in *January* and *February* 1472 or 1475 moved in a tract not very different therefrom; for, it paſs'd thro' *Urſa minor*, the thighs of *Cepheus*, the breaſt or neck of *Caſſiopeia* and the girdle of *Andromeda*; and its greateſt velocity in a day was 40°. Another comet was obſerv'd

ſerv'd in 1556, whoſe nodes *Camerarius* placed in 11° of *Libra* and *Aries* and which paſs'd near the feet of *Urſa minor*, thro' *Cepheus*, above *Caſſiopeia* and thro' the upper parts of *Andromeda*, with a very ſwift motion in its *perigæum*. And if *Regiomontanus* obſerv'd a comet in 1475 (which Aſtronomers very much doubt) there would be a ſurpriſing coincidence between theſe three comets: For, the interval between the former and middle comet would be 81 years, and between the middle and laſt comet 162 years, that ſo the revolution of the comet might be 81 years; and the hiſtory of other comets would alſo agree very well with theſe.

An Account of the Appearance of ſeveral Arches of Colours, contiguous to the inner Edge of the common Rainbow, obſerv'd at Petworth *in* Suſſex; *by Dr.* Langwith. Phil. Tranſ. N° 375. p. 241.

WHEN the primary rainbow has been very vivid, Dr. *Langwith* has obſerv'd therein, more than once, a ſecond ſeries of colours within, contiguous to the firſt, but far weaker, and ſometimes a faint appearance even of a third: Theſe increaſe the rainbow to a breadth much exceeding what has hitherto been determined by calculation; and an ingenious friend of the Dr's taking notice of this appearance, was much ſurpris'd at it, as thinking it not reconcileable with the theory.

The Dr. obſerv'd ſomething of the ſame nature, tho' not in the ſame degree of perfection, with that in the foregoing account, as follows.

March 22, 1721-22, a little before 6 o'clock in the evening, wind at N. W. by W. there was obſerv'd at *Petworth* a lively, diſtinct, primary rainbow, the inner and purple colours of which had a far greater mixture of red therein, than the Dr. could ever obſerve in Sir *Iſaac Newton*'s oblong *Spectrum*; under this was a ſpace of a breadth, conſiderably leſs than that of the limb of the rainbow, in which the Dr. could not diſtinguiſh any colours; ſtill lower was a faint interrupted arch of red, inclining to purple, which appear'd and vaniſh'd ſeveral times, while he was intent upon obſerving it.

As to this phænomenon the Dr. ſuſpects, that the extraordinary redneſs in the purple of the firſt rainbow, is owing to the mixture of the red rays of a ſecond ſeries of colours with the purple rays of the firſt; that the colourleſs ſpace conſiſted of rays, which are too weak to affect the ſight with

with diſtinct colours; and that the innermoſt broken arch was the end of a ſecond and beginning of a third iris.

As in this laſt account the Dr. obſerv'd the purple of the ſecond iris, without the other colours, ſo he obſerv'd the other colours, but not very diſtinct, without the purple; and he could never obſerve any more than one ſeries of colours near the horizon.

March 27, 1722, about a quarter before 6 o'clock in the evening, wind S. W. the Dr. obſerv'd at *Petworth* one of the fineſt rainbows he ever beheld in his life.

The firſt ſeries of colours was as uſual, only the purple had a far greater mixture of red therein, than he had ever obſerv'd in the priſmatic purple; under this was a coloured arch, in which the green was ſo predominant, that he could not diſtinguiſh either the yellow or the blue; ſtill lower was an arch of purple, like the former, highly ſaturated with red, under which he could not diſtinguiſh any more colours.

The Dr. view'd this delightful object for a conſiderable time, without that vaniſhing and returning of colours, obſerv'd in the laſt.

The order of the colours in this compounded rainbow was red, yellow, green, blue, a mixture of purple and red, green (or rather a mixture of yellow, green and blue) a mixture of purple and red.

The Dr. now began to imagine, that the rainbow ſeldom appears very lively without ſomething of this nature, and that the ſuppos'd exact agreement between the colours of the rainbow and thoſe of the priſm, is the reaſon that it has been ſo little obſerv'd.

The Dr. afterwards obſerv'd this phænomenon at *Petworth* in very great perfection, as follows.

Auguſt 21. 1722, about half an hour paſt 5 o'clock in the evening, the weather temperate, wind at N. E. the colours of the primary rainbow were as uſual, only the purple inclin'd very much to red and was well defin'd; under this was an arch of green, the upper part of which inclin'd to a bright yellow, the lower to a more duſky green, under this were alternately two arches of reddiſh purple and two of green, under all a faint appearance of another arch of purple, which vaniſh'd and return'd ſeveral times ſo quick, that he could not ſteadily fix his eyes upon it:

The order of the colours was, as follows.

1. Red

1. Red, orange-colour, yellow, green, light blue, deep blue, purple. 2. Light-green, dark-green, purple. 3. Green, purple. 4. Green, faint vanishing purple.

Here are four orders of colours, and perhaps the beginning of a fifth: For, the Dr. does not doubt, but what he calls the purple, is a mixture of the purple of each of the upper series with the red of the next below it, and the great mixture of the intermediate colours. A clergyman and four other gentlemen in company, viewing the colours attentively, agreed, that they appeared in the manner just now describ'd.

There are two things worthy observation, as they may, probably, direct us in some measure to the solution of this curious phænomenon, as,

1. That the breadth of the first series so far exceeded that of any of the rest, that as near as the Dr. could judge, it was equal to all of them taken together.

2. That he never observ'd these inner orders of colours in the lower parts of the rainbow, tho' they were often incomparably more vivid than the upper parts, under which the colours appeared. The Dr. observ'd this so very often, that he could hardly think it accidental; and if it should prove true in general, it will bring the disquisition into a narrow compass: For, it will shew, that this effect depends upon some property, which the drops retain, whilst they are in the upper parts of the air, but lose as they come lower, and are more mix'd with each other.

A Method of procuring the Small-Pox *in* South Wales; *by Dr.* Perrot Williams. Phil. Transf. N° 375. p. 262.

THE method of communicating the small-pox has been commonly practis'd by the inhabitants of *Pembrokeshire*, in *South Wales*, time out of mind, tho' by another name, *viz.* that of buying the disease, as the Dr. was inform'd by several, who procur'd the distemper by that means. A married woman in the neighbourhood of *Haverford-west* practis'd this method on her daughter, by which means she had the small-pox favourably, and was in perfect health; notwithstanding she ever after convers'd without reserve with such as had that distemper.

In order to procure the distemper, they either rub the matter, taken from the pustules, when ripe, on several parts of the skin of the arms, &c. or prick those parts with pins,

&c. previously infected with the same matter: And notwithstanding they omit the necessary evacuations, such as purging *&c.* yet as the Dr. was inform'd, they generally come off well enough; and what is remarkable is, that he could not hear of one instance of their having the small-pox a second time.

One Mr. *Owen* a gentleman of this country told the Dr. that above 20 years before, when at school, he and several of his schoolfellows, infected themselves at the same time, from the same person, and that not one of them miscarried.

The method the above mentioned gentleman took was as follows; with the back of his penknife he rubb'd the skin off his left hand, till the blood began to appear; he applied the variolous matter to that part, which by degrees growing inflam'd, about a week after, he fell into the small-pox.

The Dr. conversed with several others, who made the like experiments on themselves, and they all positively affirm'd, they never had the small-pox a second time.

A farther Account of procuring the Small-Pox; *by the Same.* Phil. Transf. N° 375. p. 264.

MR. *Owen* was about 15 years of age, when he made the experiment upon himself, and he undoubtedly had the genuine small-pox, the signs of them on his face, and the mark on his hand, where he had applied the matter, being very visible, as to put that matter beyond dispute. The Dr. had been so often assur'd of the truth of procuring the distemper in this manner, that he is entirely satisfied it has been a custom immemorial in these parts; and not only practis'd by boys at school, but also by many other grown persons of both sexes, who consequently were capable of distinguishing the small-pox from other distempers.

A young woman 23 years of age, told the Dr. that about eight or nine years before, in order to infect herself, she held 20 pocky scabs (taken from one towards the latter end of the distemper) in the hollow of her hand a considerable time; that about 10 or 12 days after, she sickened and had upwards of 30 large pustules on her face and other parts; and that she afterwards freely convers'd with such, as had the small-pox.

To make it appear, that inoculation is a preservative against receiving the small-pox a second time, the Dr. caus'd his two sons, who had been inoculated, not only to see, but even to handle a child, dying of the most malignant sort of small-pox, and yet they continued in perfect health.

Upon

Upon a very exact enquiry the Dr. found, that out of 227 who had the small-pox in the natural way, in *Haverford-west* and a neighbouring parish, since the beginning of *June* 1722, 52 had died.

A farther Account of buying the Small-Pox; *by Mr.* Richard Wright. Phil. Trans. N° 375. p. 267.

IN *Wales* the custom of buying the small-pox, as it is called, is a common practice and of very long standing, persons of unquestionable veracity and of advanc'd age affirming, that they have had the small-pox communicated to themselves this way, when about 16 or 17 years of age (being then very capable of distinguishing that distemper from any other) and that they have parted with the matter, contained in the pustules to others, which produced the same effects. Two large villages near the harbour of *Milford*, namely, St. *Ishmael's* and *Marloes*, are more famous for this custom than any other: The old inhabitants of those villages (with which they abound, being in a healthful situation) affirm, that it has been a common practice with them, time out of mind; and what was more remarkable, one *William Allen* of St. *Ishmael's*, 90 years of age, declar'd, that this practice was us'd all his time; that he very well remember'd his mother's telling him, it was a common practice all her time, and that she got the small-pox that way.

These together with the many other informations Mr. *Wright* had from almost all parts of *Pembrokeshire*, confirm him in the belief of its being a very ancient and usual practice among the common people; and to prove, that this method is still continued in *Wales*, Mr. *Wright* gives the relation of one *Joan Jones* a midwife, 70 years of age, who solemnly declar'd, that about 54 years before, having then the small-pox, one *Margaret Brown*, then about 12 or 13 years of age, bought the small pox of her; that the said *Margaret Brown* was seis'd with the small-pox a few days after, and that she had not the small-pox a second time; she farther affirm'd, that she had known this way of procuring the small-pox, practis'd from time to time upwards of 50 years; that it was lately us'd in her neighbourhood, and that she knew but of one dying of the said distemper, when communicated after the method aforesaid, the person who miscarried (a young woman about 20 years of age) having procur'd the distemper from a man, then dying of a very malignant sort of small-pox.

As

As to the manner of communicating the infectious matter to the blood, by scraping the skin with a penknife, and so rubbing in the matter; that was only the case of one particular gentleman, Mr. *Owen*, whom Mr. *Wright* heard several times positively affirm, that he bought the small-pox, when at school, and that he gave three pence for the matter contain'd in 12 pustules: It is certain that hundreds in this country have had the small-pox this way, and not one single instance can be produced of their ever having them a second time.

Experiments to prove, that the force of moving Bodies is proportional to their Velocities; by Dr. Desaguliers. Phil. Trans. N° 375. p. 269.

M. *Leibnitz* was the first who oppos'd the receiv'd opinion about the quantity of the force of moving bodies, by saying, that it was to be estimated by multiplying the mass of the bodies, not by their velocity but by the square of the velocity: But instead of shewing any paralogism in the mathematical demonstrations, made use of to prove the proposition, or any mistakes in the reasonings from the experiments made to confirm it, he uses other mediums to prove his assertions, vide *Acta Erudit. ad ann.* 1686, *p.* 162. and without any regard to what others had said on that subject, brings new arguments, which the learned Dr. *Clarke* has fully answer'd in his fifth letter to him: M. *John Bernouilli*, *Wolfius Hermannus* and others have follow'd and defended M. *Leibnitz*'s opinion and in the same manner; so that what is an answer to him, is so to them.

Polenus, Professor at *Padua*, has acted in the same manner in the experimental way, making some experiments to defend M. *Leibnitz*'s opinion, vide *Polen. de Castellis p.* 56, 57, &c. without shewing those to be false, that are made use of to prove the contrary; and an ingenious professor abroad (who was of the opinion commonly receiv'd, and in his writings had demonstrated it in the usual way, vide *Gravesande's Introduct.* Vol. 1. N° 132, confirming it with the common experiments, made in that case) happening to make some experiments, like those of *Polenus*, has drawn conclusions from them to shew the force of moving bodies to be proportional to the square of their velocity; and being wholly come over to that opinion, endeavours to deduce it from physical principles.

As

As there can hardly be any thing ſaid either new or better than has been already ſaid, to ſhew the force abovemention'd to be proportional to the maſs, multiplied into the velocity; Dr. *Deſaguliers* only repeats the ſubſtance of what others have ſaid, and makes ſome old experiments over again: But then he conſiders ſome circumſtances, that perhaps have been overlooked; and laſtly, by a new experiment, he endeavours to ſhew, what has led into an error ſome of thoſe, who defend the new opinion.

If a man with a certain force can move a weight of 50 pounds, thro' a ſpace of 4 feet, in any determinate time; it is certain he muſt employ twice that force to move 100 pounds weight, thro' the ſame ſpace in the ſame time. But if he uſe but the ſame force, he will move the 100 pounds weight, but 2 feet in the ſame time; whereas, the 100 pounds weight contains 2 fifty pounds, if each of them have 2 degrees of velocity given it, it will exactly require the ſame force, that would give one of them 4 degrees of velocity: Hence it appears, that the force is proportional to the maſs, multiplied into the velocity.

Experiment 1. Let the balance AB Fig. 14. Plate VII. whoſe *fulcrum*, or center of motion, is at C, be ſo divided, that the *brachium* AC be but $\frac{1}{4}$ part of the *brachium* CB; it is known to all converſant in mechanics, that a weight of 100 pounds at A, will keep in *æquilibrio* a weight of 25 pounds, ſuſpended at B, where it will have a velocity 4 times greater than that of the weight at A: For, not only when the balance is horizontal, there will be an *æquilibrium*; but when the balance is put in motion, it will return to an *æquilibrium* in an horizontal poſition; the equal and contrary forces, applied at each, deſtroying one another; Whereas, if the forces were as the maſs, multiplied into the ſquare of the velocity, the 25 pounds weight ſhould have been ſuſpended at D, only twice as far from C, as the weight at A; and in general let the make of the engine be what it will; let the mechanical powers be combined in any manner; when 2 heavy bodies, by means of a machine, act upon each other in different directions, if their velocities are reciprocally as their maſſes, they will deſtroy each other's forces and come to reſt.

As this is true in reſpect of mechanical powers, ſo it is alſo in reſpect to the ſhock or blow, given by falling bodies: An heavy body, falling with an accelerated motion, goes thro' a ſpace of 1 foot in $\frac{1}{4}$ of a ſecond, and acquires a velocity, which

would

would carry it 2 foot in the same time with an uniform motion; the same body falls thro' a space of 4 foot in ½ a second and acquires a velocity, that would with an uniform motion carry it 8 foot in ½ a second: Therefore, as the time of the fall thro' a space of 4 foot is twice the time of a fall thro' one of 2 foot, the velocity in the latter case is double that of the former; and consequently, the blow that the body will give will be double.

Exp. 2. Let the weight P of one pound Fig. 15. be placed in the scale, suspended at the extremity A of the balance A B, which bears upon the *gnomon*, or iron supporter *kbi*. Then if the weight C be let fall from D, or 1 foot, it will by its stroke on the end of the beam B, raise up the opposite end A with the weight P, so high, that the spring *gh* will fly from the button *i*, which kept it streight and upright before the shock. If the weight P be of 2 pounds, it cannot be raised by the fall of C from any height less than F or 4 foot: Whereas, if the force of the shock were proportional to the space, without any regard to the time, as M. *Leibnitz* and his followers affirm, P ought to be raised, when C falls only from E, or 2 foot, which never happens; or if the stroke were proportional to the mass, multiplied into the square of the velocity, when C falls from F, then P might weigh 4 pounds; whereas the experiment will never succeed under those circumstances.

It is objected, that the blow cannot be always direct, and that the string which goes thro' the hole in the falling weight to guide it in its fall, causes a sensible friction; and therefore that something of the force is thereby lost. But we are to observe, that if that were all, there would be no need of raising up C in the second case quite up to F, instead of E; whereas, in fact it must always be raised beyond F, to allow for the friction, that hinders it from producing a double effect in falling from F. For, if the experiment be repeated an hundred times, the weight P, when of 2 pounds, will never be raised by letting C fall from any place between E and F.

Exp. 3. If (in order to avoid friction) instead of a blow struck upon the end B, by the falling body, the said body C be fastened to a pretty long string, tied to the button *m*, as at *c*, and first lifted up 1 foot and then let fall; so that in falling 1 foot, it may pull down B, and lift up A with the weight P of 1 pound; whenever P is 2 pounds, C must fall from a height, greater than *f* or 4 foot; otherways it will not

ot raiſe the *brachium* A, eſpecially, if it be let fall between and *f*.

Exp. 4. The Dr. took the weight C of 17 ounces *Troy*, which was a round ball of lead, with a hole thro' the middle, and having paſſed the ſtring N—X thro' it, before it was faſtened to the hook X, he placed the whole machine in ſuch a manner, that the ſtring being ſtretched by the weight N, went thro' the hole of the weight C, and likewiſe thro' the hole of the *brachium* B, upon which C lay, without touching the ſides of the hole either in the weight or balance; then having put such a weight P in the oppoſite ſcale, as C, falling from the height of 1 inch, was able to raiſe high enough, to let looſe the ſpring *g h* from the button *i*; he added to P another weight equal to it; and then letting C fall along the ſtring, that guided it, from an height of 2 inches, then of 3, and then exactly of 4, it would not raiſe the double weight P to the former height; but falling from 5 inches or a little higher, it raiſed it up.

Exp. 5. Leaving every thing, as it was before, he changed the weight C for another leaden ball of twice the weight, which falling from 1 inch, raiſed the double weight P to the uſual height; then changing the weight P in any proportion, whatever height was required for the heavieſt ball C or C 2, to fall from, in order to raiſe the weight at P; more than 4 times the height was required for the firſt ball C, to raiſe the ſame weight ſo high, as to let looſe the ſpring.

Exp. 6. He tried the experiment with the weight C, ſuſpended at the ſtring *mc* (as in *Exp.* 3.) and a fall from a height of 5 or near 5 inches, was required to raiſe double the weight in the oppoſite ſcale, that a fall from 1 inch would raiſe; only here the height above 4 inches was not ſo great, as in the former experiment, the friction being ſomething leſs. Then he ſuſpended the large ball C or C2 by the ſtring *mc*, and when by falling 1 inch it raiſed the weight P, the ſmall weight C could not produce the ſame effect, without falling from a greater height than 4 inches.

It is here to be obſerved, that which way ſoever theſe experiments are tried, the objections, ariſing from the friction, do noways ſerve to confirm the new opinion, becauſe they ſhew, that upon account of the friction, the heights muſt be ſomewhat more than in a duplicate proportion of the velocities, but never leſs, to give a blow with the ſame body in proportion to the velocity.

 That

That the *momentum* of bodies is in proportion to the maſ multiplied into the velocity, is alſo moſt evidently ſhewn fro the congreſs of elaſtic bodies, as has been demonſtrated by S *Iſaac Newton* in his *Principia* in the *Corollaries* to the laws motion. Dr. *Deſaguliers* had often tried the experiments the mentioned with balls of ivory and balls of glaſs; and ſome them with two balls of ſteel, of two ounces each; and he foun every thing anſwer, allowing for the want of perfect elaſtici in the bodies. But now, as the objections to the received op nion were renewed, the Dr. was willing to repeat the exper ments with the utmoſt accuracy; and therefore, as ivory bal are not equally denſe in all their parts and glaſs balls brea after 2 or 3 ſtrokes; he cauſed ſteel balls to be nicely turn and made as hard (as the workmen call it) that is, as elaſtic poſſible, and their weights were preciſely as follows; 2 balls 12 ounces *Troy* each, 1 of 6 ounces, 1 of 3, 1 of 2, and 1 8 penny-weight; then making pendulums of theſe balls, an hanging them upon the machine, contrived by *Mariotte* for th congreſs of bodies, and improved by Dr. *Graveſande*, vi *Introd.* N° 171. Vol. I. he meaſured 57 inches and ¼ betwe the center of ſuſpenſion and the center of gravity of the bal and then every degree of the circle they deſcribed in their oſci lation was 1 inch, and the degrees being marked upon a line chords on a braſs ruler above the balls, by their ſtrings ſucce ſively covering the croſs lines of diviſion, the degrees, the bal fell from, and thoſe to which they roſe, were very diſcernib to an eye placed at a convenient diſtance.

Exp. 7. He took the 2 balls 12 and removing each fro the loweſt point of their equal and reſpective circles, up to inches or 4 degrees, he let them fall, ſo that they met at bo tom, and were both reflected again to 4, the place from when they fell.

Exp. 8. Every thing elſe being as before, inſtead of 1 the balls 12, he took the ball 6; then letting 6 go from 8 d grees, and 12 from 4; after reflection 12 was driven up aga to 4, as before.

Exp. 9. The ball 3 falling from 16 degrees met the b 12, that fell ſtill from 4, and after reflection 12 went up aga to 4.

Exp. 10. The ball 2 falling from 6 degrees and 12 from 12 was reflected to 1; and when 2 fell from 12 degrees, a the ball 12 from 2, the 12 was reflected to 2.

Ex

Exp. 11. The ball of 8 penny weight (which weighed but $\frac{1}{30}$ of the ball 12) falling from 15 inches or degrees, raiſed up 12 (that fell from $\frac{1}{2}$ a degree) to the ſame place again.

In all theſe experiments the error, or want of perfect reflexion, was greater in the ſmall balls than in the large ones, on account of their going thro' a greater arch of a circle, whereby they deviated more from a cycloid than the large balls; as likewiſe on account of the reſiſtance of the air, which muſt be greater, becauſe of the ſmall balls going thro' a larger arch, moving with more velocity, being ſuſpended by a ſtring, as thick as that of the large ones, and having more ſurface in proportion to their weight. But all the errors do not bring the phænomena any thing near what they ought to be, if the force of the bodies were, as the ſquare of the velocities, multiplied into their maſſes; for, then the ball 12 would have been driven to heights very different from what it roſe up to.

In the 8th experiment, the ball 12 ſhould have riſen to near 5 inches and $\frac{3}{4}$; for, the ball 6, falling with the velocity 8, muſt have had its force $= 8 \times 8 \times 6 = 384$; and then that the ball 12 might have the ſame force, or quantity of motion, it muſt riſe near to 5, 7; becauſe $5,7 \times 5,7 \times 12 = 389,88$.

In the 9th experiment, 12 ſhould have riſen to 8; for, the ball 3 muſt have had its force $= 16 \times 16 \times 3 = 768$; and if 12 received its whole force, it muſt have riſen to 8; becauſe $8 \times 8 \times 12 = 768$.

In the 2d part of the 10th experiment, 12 ſhould have riſen to near 5; becauſe $12 \times 12 \times 2 = 288$ and $5 \times 5 \times 12$ is but 300.

In the 11th experiment, the ball 12 (30 times heavier than the little one) muſt have gone to 2 inches $\frac{3}{4}$; becauſe the *momentum* of the little ball being equal $15 \times 15 \times 1 = 225$, that of the ball 12 muſt be $= 2,75 \times 2,75 \times 12 = 226$, &c.

It may be here alledged, that the *momentum*, with which the large ball comes upon the ſmall one, ſhould be ſubſtracted; but that will not mend the matter much: Tho' indeed, the difference will be leſs. For,

In the 8th experiment, if we ſubſtract $4 \times 4 \times 12 = 192$ from 389,88, there will remain 197,88, and the ball 12 will go but to 4: But then in experiment 9. if we ſubſtract the ſame N° 192 from 768, we ſhall have 576, which would carry 12 to near 7 degrees; becauſe $7 \times 7 \times 12 = 588$.

In the 10th experiment, there is only 48 to be ſubſtracted; and in the 11th only 15; and therefore, the velocity of 12

will very much fall ſhort of what is agreeable to the new opinion.

After the experiments made and what has been ſaid, till theſe conſequences are overthrown, no notice ought to be taken of any objections, or new experiments.

Animadverſions on ſome Experiments, relating to the Force of moving Bodies; *with two new Experiments on the ſame Subject; by the Same.* Phil. Tranſ. N° 376. p. 285.

IN the preceeding *Tranſaction* Dr. *Deſaguliers* demonſtrated by reaſon and experiments, that the *momentum* or force of moving bodies is always proportional to their maſs, multiplied into their velocity, and this is the opinion of the greateſt part of mathematicians and philoſophers. Now he comes to conſider the experiments, that have led ſome ingenious men into an error, as to this propoſition.

Polenus in his book *de Caſtellis* p. 56. N° 118. gives an account of his experiments, relating to this matter, in theſe words: 'I took a veſſel that had therein congealed tallow 6 inches deep, and fixed it to a level floor, in ſuch a manner, that the ſurface of the tallow, which was flat, ſhould every where be equally diſtant from the floor. I had cauſed to be made two balls of equal bigneſs; the one of lead, the other of braſs, the laſt of which was ſomewhat hollow in the middle, that it might weigh but 1 pound, whilſt the other weighed 2: Suſpending theſe balls from the cieling by threads, in ſuch a manner, that the lighter ball hung over the ſurface of the tallow, from twice the height that the heavier ball did; I cut the threads, and the balls falling perpendicularly upon the tallow, by their fall made pits in the tallow, that were preciſely equal; the ball of 1 pound from the beginning of its fall till it came to reſt, going thro' a ſpace, expreſſed by number 2, produced an effect, equal to that, which the two pound ball produced, in falling thro' a ſpace, expreſſed by number 1. It follows, therefore, that we may look upon it as a ſettled truth, that the active forces (*vires vivas*) of falling bodies are then equal, when their proper weights are in a reciprocal ratio of the ſpaces, which the ſaid bodies deſcribe by their fall; and becauſe thoſe ſpaces are in the ſame ratio, as the ſquares of the numbers, expreſſing the velocities; it appears by the experiment, that the active force (*vim vivam*) of the falling body, is that which is made up of the body itſelf, multiplied into the ſpace, deſcribed in the fall, or into the

'ſquare

square of the number, that expresses the velocity of the body, at the end of the motion. This experiment I did not only make once, but several times, changing the balls, the distances and the body on which they fell; as for example, making use of clay, or soft wax: And notwithstanding these various ways of trying the experiments, the effects were constantly the same; which made me easily conclude, that there was always the same reason in nature for this phænomenon.'

Thus far *Polenus*; whose mistake lies in this, *viz.* That e estimates the force of the stroke of the falling balls, by e depth of the impression made in the tallow, clay, wax, any yielding substance. But we must consider, that when o bodies move with equal forces, but different velocities, at, which moves the swiftest, must make the deepest imression; whilst the slowest body communicates its motion the clay round about; and therefore, does not strike in so eep, as the swifter body, which puts in motion few parts of e clay, besides those that are before it, and which parts ve so much less time to oppose this body's motion, as its elocity is greater than that of the other.

To make this plainer, let us suppose a door half open, nd moving very freely on its hinges, if a pistol be fired ainst it, the ball will go thro' the door, without moving it ut of its place: But if we take a large weight of lead and row it against the same door, with the same force as the istol bullet moved, the door will be removed from its posion, and carried out of its place on its hinges by the stroke, ecause in the first case, the motion of the bullet is commuicated but to a few parts of the door, and in the last case, it diffused all over it. Nay, the door will be moved by the roke, even tho' there should be a prominent part in the ad, that should be no bigger than a pistol-bullet, in order strike the door upon no more of its surface than the bullet ad done.

For illustrating this farther the Dr. contrived the following periment.

He caused a machine to be made (as represented by Fig. 16. late VII.) consisting of a base of wood A B, which could be t horizontal by means of three screws, such as SSS; upon is board or base there stood upright two parallel boards, bout four inches wide and four inches asunder, with the bow-piece E F, sliding behind one of them, so as to raise

its

its upper end F to any height deſired. Between theſe board ſix ſquare frames of wood CC, *&c.* with paper extende thereon, could ſlide in, in an horizontal poſition: Theſe pape diaphragms being thus placed, he ſuſpended an ivory ball o about 1 inch and ½ in diameter, weighing ſomething more tha 1 ounce and ½, by a ſhort thread under F; ſo that its center o gravity hung 4 foot over the firſt diaphragm; then cutting th thread, the ball fell upon the paper, and by its perpendicula ſtroke broke thro' that diaphragm and the 3 next under it Then putting ſo much lead into the ball above mentione (which was made hollow for that purpoſe) as to make it weig twice as much as it did before; and bringing down F, to let fall but from 1 foot; by its fall it broke thro' 2 diaphragm only. Making the experiment ſeveral times with differe heights; but ſtill keeping the proportion in height of 4 to when the balls were as 1 and 2, the heavy and ſloweſt ba broke thro' but half the number of papers. It happened ſom times, it is true, that there was ſome little difference, wh the papers were not equally ſtrong, or equally ſtretched; b the ſwifteſt ball always broke thro' more papers than the ſlo one.

Now, tho' this does at firſt ſeem to confirm *Polenus*'s theor yet when duly weighed, it proves no ſuch thing: For, t lighter ball does not break thro' more papers; becauſe it h more force, or a greater quantity of motion; but becauſe ea diaphragm has but ½ the time to reſiſt the ball, that falls with double velocity; and therefore, their reſiſtance being as t time, as many more of them muſt be broken by the ſwift ba as by the ſlow one.

What has been ſaid, in this and the preceeding *Tranſacti* ſeems to be a full anſwer to all objections, that allow the for of moving bodies and their quantity of motion to be the ſam But as there are now ſome philoſophers, who diſtinguiſh th force from the quantity of motion, the Dr. adds ſomethi more for the clearing up of that point.

As the Dr. takes it, they call *vis viva* a force, whoſe effe is ſenſible, as the force of gravity, when it accelerates bodies their fall; and *vis mortua*, a force, which being deſtroye produces no ſenſible effect; as the force of gravity acting up a weight in one ſcale of a balance, when the weight cannot d ſcend by reaſon of a counterpoiſe in the other ſcale. But ce tainly no man, that conſiders the thing attentively, would ma that diſtinction: However, ſince *Polenus* allows, that

quant

[q]uantity of motion in bodies is as the maſs, multiplied into [t]he velocity, or M V; but ſays, that the force, with which [t]hey act, which he diſtinguiſhes by the name of *vis viva*, is, [a]s the maſs, multiplied into the ſquare of the velocity, or M V [V]: The Dr. made the following experiment to ſhew, that his [n]otion is inconſiſtent; tho' all the phænomena of unequal [w]eights, applied to a *ſtatera*, ſo as to make an *æquilibrium*, [m]ight ſerve for that purpoſe; if it had not been objected, that [t]he particular conſtruction of the machine, hindered it from [a]greeing with the ſuppoſed theorem, *viz.* that the force is as [t]he matter, multiplied into the ſquare of the velocity.

Exp. Let two balls A and B (Fig. 17.) be joined by a [ſ]tring, which, going thro' the ſmooth hole C of an even table, [a]nd under the pulley P, ſuſpends a weight W. It is plain, that [u]pon letting go the balls A and B, from the places A and B, [t]hey will move towards C with the ſame force; becauſe each of them will be drawn towards C by half the force of the weight W, whether the balls be equal or unequal.

1. The balls, being of ivory and weighing two ounces each were, at the ſame inſtant of time, let looſe from A and B, each diſtant 12 inches from C, and both came to C at the ſame time: Here the equal forces will agree with the product of the maſſes into the velocities, or into the ſquares of the velocities; becauſe $A \times 12 = B \times 12$, as well as $A \times 144 = B \times 144$.

2. If A be taken of 4 ounces weight and let go from D, or 6 inches; whilſt B, ſtill equal to 2 ounces, moves from 12 inches; both bodies will again meet at C: Therefore, here the equal forces muſt be expreſſed by the maſſes into the velocities, and not into their ſquares; for, tho' $A \times 6$ be equal to $B \times 12$ $(4 \times 6 = 2 \times 12)$ $A \times 6 \times 6$ or 144 is but half of $B \times 12 \times 12$ or 288. Whereas, if the forces had been as *Polenus* affirms, B ſhould have been let looſe only from 8,4 inches.

3. When A is 6 ounces, it is let looſe only from E, or 4 inches, to meet at C with B let looſe from 12: For, then $A \times 4 = B \times 12$; whilſt $A \times 4 \times 4$, or 96 is 3 times leſs than $B \times 12 \times 12$, or 288: So that, according to *Polenus*, B muſt have been let looſe from 7: But in that caſe it comes ſooner to C than A.

N. B. The weight W muſt be greater than that of both balls, leaſt the friction of the table ſhould ſpoil the experiment.

An

An Account of an Experiment made to aſcertain the Pr*portion of the expanſion of the Liquor in the* Thermomete *with regard to the Degrees of Heat; by Dr.* Brook Ta lor. Phil. Tranſ. N° 376. p. 291.

IT has been generally ſuppoſed, tho' not proved, that t expanſion of the liquor in the thermometer is proportion to the increaſe of heat. To determine this matter with ce tainty, Dr. *Taylor* made the following experiment.

He provided a good linſeed oil thermometer, which h marked with ſmall diviſions, unequal in length, but equal a cording to the capacity of the tube in the ſeveral parts of it, all thermometers ought to be graduated. He likewiſe provid two veſſels of thin tin, of the ſame ſhape, and equal in cap city, containing each about a gallon. Then (obſerving in eve trial, that the veſſels were cold, before the water was poure in, as alſo that the veſſel he meaſured the hot water with, wa well heated thereby) he ſucceſſively filled the veſſels with 1, 3, &c. parts of hot boiling water, and the reſt cold; and laſt with all the water boiling hot; and in every caſe he imme ſed the thermometer into the water, and obſerved to what ma it roſe, making each trial in both veſſels for the greater acc racy: And having firſt obſerved, where the thermometer ſtoo in cold water, he found, that its riſing from that mark, or th expanſion of the oil, was accurately proportional to the quan tity of hot water in the mixture, that is, to the degree heat.

An Account of the Rattle-Snake; *by Mr.* Dudley. Phi Tranſ. N° 376. p. 292.

THE rattle-ſnake is reckoned by the *Aborigines* to be th moſt terrible of all ſnakes, and the chief of the ſerpen kind; what cauſes the terror of them, is, undoubtedly, the mortal venom, and the enſign of it is their rattle; and it i moſt certain, that both men and beaſts are more afraid of the than of other ſnakes; and while the common ſnake avoids man, this will never turn out of the way.

There are three ſorts of this ſnake, that are diſtinguiſhed b their colour, *viz.* a yellowiſh green, a deep aſh-colour, and black ſattin.

The eye of this reptile has ſomething ſo ſingular and terrible that there is no looking ſtedfaſtly upon him; one is apt almo to think, they are poſſeſſed by ſome Demon.

A rattle-

A rattle ſnake creeps with his head cloſe to the ground, and s very ſlow in moving; ſo that a man may eaſily get out of his vay; his leaping and jumping to do miſchief, is no more than xtending or uncoiling himſelf: For, they do not remove their whole body, as other animals do, when they leap; ſo that a nan is in no danger of them, if his diſtance be more than their ength; nor can they do any harm, when they are in their ordiary motion, till they firſt coil and then extend or uncoil themelves, which are both done in a moment's time.

When a rattle-ſnake reſts or ſleeps, he is coiled; and they re obſerved to be exceeding ſleepy.

The tail of the rattle-ſnake is compoſed of joints, that lap ver each other, ſomewhat like a lobſter's tail; and the ſtrikg them upon each other forms that noiſe, which is ſo terrible man and beaſt: The fierceſt noiſe is obſerved to be in fair lear weather; for, when it is rainy, they make none at all: or which reaſon the *Indians* do not care to travel in the woods, time of rain, for fear of being among theſe ſnakes, before ey are aware. Another circumſtance of their rattling has een obſerved, to wit, that if a ſingle ſnake be ſurpriſed and attles, and there happen to be others near him, they all take e alarm, and rattle in like manner.

Mr. *Dudley*, would not pretend to anſwer for the truth of very ſtory he had heard of their charming or power of faſciation; yet he was abundantly ſatisfied from ſeveral witneſſes, oth *Engliſh* and *Indian*, that a rattle-ſnake will charm both quirrels and birds from a tree into his mouth. Mr. *Dudley* as told by one of undoubted probity, that as he was in the oods, he obſerved a ſquirrel in great diſtreſs, dancing from e bough to another, and making a lamentable noiſe, till at ſt he came down the tree and ran behind a log; the perſon oing to ſee what was become of him, ſpied a large ſnake that ad ſwallowed him.

And Mr. *Dudley* is the rather confirmed in this relation, beauſe his own brother, being in the woods, opened one of theſe akes and found two ſtriped ſquirrels in his belly, and both of em head foremoſt. When they charm, they make a hoarſe oiſe with their mouths, and a ſoft rattle with their tails, havg the eye at the ſame time fixed on the prey.

Their common food is toads, frogs, crickets, graſshoppers d other inſects, but principally ground mice; and the rattleake again ſerves for food to bears; and even hogs will eat em without harm.

They are viviparous and generally bring forth about 12, an that in *June*. A friend of Mr. *Dudley*'s in the country, bein deſirous to diſcover the nature and manner of the generation the rattle-ſnake, gave him the following account, *viz.* abou the middle of *May*, the time when the rattle-ſnakes firſt com abroad, he took and opened one of them and found in the *ma trix* 12 ſmall globes, as big as a common marble, in colou like the yolk of an egg; in three or four days more he took an opened another, and then plainly perceived a white ſpeck in th center of the yellow globe; in three or four days more he di ſected a third, and diſcovered the head of a ſnake; and in few days after that, $\frac{3}{4}$ of a ſnake was formed, lying round in coil. In the latter end of *June* he killed an old one and too out perfect live ſnakes ſix inches long. In *September*, when th old ones take their young in and carry them to their dens, the are not quite a foot long. They couple in *Auguſt* and are th moſt dangerous.

As to other ſerpents or poiſonous reptiles Mr. *Dudley* coul not affirm; but he was ſatisfied the rattle-ſnake does not traje his poiſon; and that unleſs the ſkin be firſt broke or an incisi made with his teeth, his venom can do no harm; for, his frien aſſured him, that he had made an experiment of it in this ma ner; he took the but-end of his gun; and ſet it upon four five of them; and after they had bit it and left ſeveral drops their poiſon, he wiped it off with his hand without any harm.

There are ſeveral remedies for the ſting of a rattle-ſnak among others, that which is much made uſe of, is a root, ca led blood-root, ſo called, Mr. *Dudley* ſuppoſes, from the colou of the root, and the juice, which is red like blood. It grow in great abundance in the woods; they bruiſe the root and bin it over the place that is bit, to prevent the poiſon going farthe at the ſame time ſcarifying the place affected: Some of the ro is likewiſe boiled, and the perſon bit drinks the water.

The rattle-ſnakes are generally from three to five foot lon and commonly exceed not 20 rattles; and yet Mr. *Dudley* w told by a man of credit, that he killed a rattle-ſnake, that ha between 70 and 80 rattles, with a ſprinkling of grey hairs, lik briſtles, over his body; he was full five foot and $\frac{1}{2}$ long, an as big as the calf of a man's leg.

They caſt their ſkins every year, ſome time in *June* and tu it inſide out, when they throw it off. It has alſo been obſerve that the ſkin covers not only the body but likewiſe the head an eyes.

The

They generally den among the rocks in great numbers toge-er; the time of their retiring is about the middle of *Septem-r*, and they do not come abroad, till the middle of *May*, hen the hunters watch them, as they come out a ſunning, and ill them by hundreds.

ome *Obſervations on* Vipers; *by Dr.* Sprengell. Phil. Tranſ. N° 376. p. 296.

AT *Milan* Dr. *Sprengell* found a viper-catcher, who ſeldom was without 60, or more, vipers alive, kept together in a ack room open a top; he had them from all parts of *Italy* and ld them dead or alive, according to the uſes they were de-gned for. Having one day got a female viper, big with young, e catched ſome mice, and throwing in one at a time; amongſt ll that number of vipers (which were upwards of 60) there as none of them that in the leaſt concerned himſelf about the ouſe, till the pregnant female viper and the mouſe interchanged yes; whereupon the mouſe ſtartled; but the viper raiſed her ead, and turn'd her neck into a perfect bow, the mouth open, he tongue playing, the eyes all on fire and the tail erect: The nouſe ſeem'd ſoon recover'd of his fright, would take a turn or wo, and ſometimes more, pretty briſkly, round the viper, and iving at times a ſqueak, would run with a great deal of ſwift-eſs into the chops of the viper, where it gradually ſunk down he gullet. All this while the viper never ſtirred out of her lace, but lay in a ring.

It is to be obſerved, that no viper will feed, when confined, xcept a pregnant female viper: The Dr. ſaw the ſame thing at *Bruſſels*, where a ſoldier had catched a large viper big with young.

A ſmall pig of 9 or 10 days old, was bit by the viper in the tail, and in four minutes time the tail was chopp'd off, the pig appearing to be ſick and dizzy, and the remaining part of the tail ſwell'd; but the bleeding, the Dr. ſuppoſes, ſav'd it; for, it was well again next morning: The ſame happened to ano-ther pig, which was bit in the fore-foot, and ſtaying ſeven mi-nutes after the bite, the leg was cut off about two inches above the bite. After theſe two, three more were bit in ſeveral places, two of which died that night, and the third recovered, having given it, about five or ſix minutes after, 10 grains of emetic tartar.

This the Dr. tried afterwards upon dogs, bit by vipers and he found, that they all recovered upon the emetic tartar.

Observations on the Figures of Snow; *by Dr.* Langwit
Phil. Transf. N° 376. p. 298.

ON the 30. of *January*, 1723, something past 9 o'clock the morning, weather cold, wind south westerly, but n very high; barometer above 30 inches, Dr. *Langwith* observ that pretty phænomenon of the star-like snow; and tho' up comparing his observations with those of *Descartes*, Dr. Gre and Mr. *Morton*, he found he had but little to add upon th subject; yet he observed with a great deal of pleasure the pr gress of nature in this sort of crystallization, an account which is, as follows.

ABCDEFG, Fig. 18. Plate VII. represent the sever figures of snow: A and B are the most simple figures, of whi the former is a roundish pellet of ice; the second a small o long body, with parallel sides, and often as fine as a hair: O this latter kind the flakes of snow chiefly consist; and tho' th look white to the eye, yet when viewed with a small magnifi of a microscope, they appear like so many transparent need of ice, thrown together, without any manner of order.

The next figure of snow is C, in which the pellet has sh out six of those small bodies of equal length, and set at equ angles: Of this kind the Dr. observed a great many.

The next step in the crystallization is D, in which those b dies are lengthened and have shot out a great many more fro their sides, at equal angles, but unequal lengths, as growin continually shorter and shorter, till they terminate in a point He measured some of these, and found them to be $\frac{1}{4}$ of an inc n breadth. He observed but very few of them perfect; fo the collateral shoots were so exquisitely fine, as to be liable t be broken in their fall, or confounded together by the least de gree of heat.

Of the next kind E, he observed a vast many, which, bein examined by the microscope, plainly appeared to be nothing but the former sort in disorder. The edges of these were in ge neral very irregular; but some of them happened to be so in dented, as to look like the jagged leaves of plants.

The next kind F had 12 points regularly disposed, and pro bably might consist of two of the former, so joined together, a to cut their angles equally.

Perhaps also those Mr. *Morton* describes (represented by G as consisting of *radii*, which, instead of terminating in a point grow bigger, as they advance from the center, might be form'd

from

m two of the kind C, so join'd at the center as to cut each her's angles unequally; for, in the progress of the crystal- ation, these *radii* would quickly unite.

Lastly, that sort, which *Descartes* compares to roses, and which he has given a figure in his treatise of meteors, may nothing but the kind E, when the points are rounded off, being gently thaw'd.

The Dr. proposes these things only by way of conjecture; ecause as the small drops of water may be impregnated with ry different particles in the air, it is not easy to determine, hether these figures may not be the result of a crystalli- ation, quite different from the former.

The Dr. observ'd but very few figures of 12 points, and ose mostly imperfect in one respect or other.

bservations for four Years on the Aurora Borealis, *at* Lyn-Regis *in* Norfolk. Phil. Transf. N° 376. p. 300. *Translated from the* Latin.

THE author for four years together made some observations on the *Aurora Borealis*, at *Lyn-Regis* in *Norfolk*; e was not aware of the surprising phænomenon the sixth of *March* 1715-16; which made him the more sollicitous to atch the subsequent phænomena.

He very carefully delineated the first observation, which is ere subjoin'd, as soon as he made it; and therefore he takes to be but little wide of the truth: The rest of the descrip- ons are very accurate. The cause of these coruscations is ot, he thinks, hitherto sufficiently discovered.

Friday Sep. 5. 1718. About 10 o'clock in the evening, he phænomenon (represented by Fig. 19. Plate VII.) was bserv'd at *Lyn-Regis* to the north.

Saturday Sep. 6. About 8 and 10 o'clock in the evening, everal columns of light were observed like those represented *aa* in Fig. 19. but not so bright as the pyramids observ'd he night before, which were carried towards the east, but hese towards the west.

Thursday Sep. 11. A great many columns of the same kind ere again observ'd moving to the west.

Saturday Sep. 13. About 11. o'clock at night the *Aurora Borealis* was brighter and higher, and with longer pyramids han it was hitherto observ'd to have: This nighr it shone o bright, that one could read by it.

Saturday

Saturday Oct. 11. Between 10 and 11 o'clock at night the *Aurora Borealis* was brighter than ever, with pyramidal coruſcations interſpers'd; and not much unlike the phænomenon, obſerv'd *September* 5, it was ſo bright that one might read by it.

Friday December 19. Between 8 and 9 o'clock at night, ſeveral rays of light, together with the *Aurora Borealis*, were obſerv'd to ariſe out of a black cloud, as it were; but that it was really no cloud appeared from the ſtars, being plainly ſeen thro' it; yet what elſe it was, could not ſo eaſily be determined: But what was chiefly obſervable was the very ſurpriſing motion of theſe lights: Theſe rays were continually in motion, and conſtantly ſhifting their places, directing their courſe now this way, and now that way, backwards and forwards, and ſometimes ſucceſſively advancing in the ſame direction, and ſometimes again impinging againſt each other with a tremulous and vibrating motion, and almoſt an incredible ſwiftneſs: Some rays were obſerv'd to extend as far as the zenith: The light was often obſerv'd to collect itſelf into a body, and ſo exhibit a ſurpriſing brightneſs, tinged with the colours of the rainbow; and again it was obſerv'd to dilate itſelf. This evening the moon ſhone bright.

Thurſday March 12. 1718-19. Between 10 and 11 o'clock the *Aurora Borealis* was obſerv'd again.

Friday March 27. The *Aurora Borealis* was obſerv'd again with various oblique radiations, as repreſented Fig. 20.

Monday October 26. 1719. It was likewiſe obſerv'd this evening between 7 and 8 o'clock.

As alſo *Monday Nov.* 9. 1719.

Sunday January 31. 1719-20. The *Aurora Borealis* was obſerv'd this evening from 7 till 10 o'clock higher than ever it had been before, and extending over half the heavens from eaſt to weſt; it was almoſt cover'd with ſeveral interſpers'd coruſcations, conſequently ſo bright that one might diſtinctly read by it.

Saturday Sep. 17. 1720. The ſame appearance.

Monday January 6. 1720-21. Between 7 and 8 o'clock at night the *Aurora Borealis* was again obſerv'd, with pyramidal coruſcations, ariſing on all hands from the zenith, as from a center, and almoſt reſembling an *umbrella*.

Sunday January 12. 1720-21. The ſame appearance.

Monday September 11. 1721. About 9 o'clock at night.

Wedneſday

Wednesday Sep. 5. 1722. From 10 o'clock till ½ an hour after 10.
Wednesday October 3. About 9 o'clock at night.
Thursday October 4. About 10 o'clock at night.
Sunday December 23. 1722. About 8 o'clock at night.

An Account of a reflecting Telescope, made by Mr. Hadley, *together with the Description of a Machine, contriv'd by him for applying it to Use.* Phil. Transf. N° 376. p. 303.

THE instrument consists of a metalline *speculum*, about six inches in diameter: The radius of the sphere, on which its concave surface was ground, is 10 foot, 5 inches and ¼; and consequently its focal length is 62 inches ⅝: The back has a hollow screw made at its center, to receive the end of the handle, which is screw'd on, whenever the metal is to be mov'd, in order to avoid sullying its polish'd surface by handling.

This object metal A (Fig. 1. Plate VIII.) is placed in one end of an octangular tube BB, about six foot long, and something wider than what is sufficient to receive the metal, dyed black on the inside: About six or seven inches in length of the three uppermost sides of the tube C (towards that end, at which the metal is placed) are separated from the rest, and open with two hinges, to make room for the metal to be put in and taken out. The end of the tube is clos'd by an octangular piece of board D, which has an opening *d*, about ⅝ of an inch broad, from the top down to a little below the center, to give room for the above-mentioned handle, when the object metal is lifted into or out of the tube; at other times it is clos'd with a sliding shutter: The metal is placed in such a manner, as to have its axis coincide with that of the tube, by the means of three small buttons, fix'd to the inside of the tube, having their hinder ends all in the same plane, to which this axis is perpendicular: Two of these appear at *aa*, the third, being at the middle of the bottom of the tube, is not seen: The foreside of the metal rests against these buttons in three points of its circumference, nearly equidistant from each other, and is held to them by three screws (one of which appears at *b*) which run thro' the octangular board at the end of the tube and bear against the back of the metal (in three points, which directly answer those three on the foreside)

foreſide) with juſt ſo much force as is requiſite to keep i ſteady in its place; they muſt not be ſcrew'd harder againſ the metal for fear of bending it, which (tho' it is half an inch thick) a very little force is ſufficient to do. When the inſtru-ment is not us'd, theſe ſcrews are looſened and the object metal is taken out and laid by, to prevent its tarniſhing.

The oval plane is compos'd of a plate of the ſame metal with the great *ſpeculum*, about $\frac{1}{5}$ or $\frac{1}{6}$ part of an inch thick ſolder'd on the back to another of braſs; its breadth is ſome-thing leſs than $\frac{1}{2}$ an inch, and is to its length, as 1 to $\sqrt{2}$. At one end of the oval, the braſs plate projects a little beyond the other, and has a ſcrew cut thro' it in that part, as likewiſe another directly againſt the center of the foreſide. The other end is cypher'd away on the backſide, that it may in-tercept as few of the rays, in their paſſage towards the object metal, as is poſſible. The two ſcrew holes in the back ſerve to fix this oval A Fig. 2. to a braſs arm B, which is faſtened at the other end into a ſlider E E, Fig. 1, 2. This ſlider is of an equal thickneſs with the ſide of the tube, and has a groove G G Fig. 1. cut for it in that ſide, parallel to the axis, and long enough to give room for its motion, to ſet the two *ſpecula* at the different diſtances, which the ſeveral eye-glaſſes require. It reſts on the inſide againſt two thin ledges, faſtened within the tube, along the ſides of the groove. On the outſide it is kept in its place by a ſliding ſhutter, not ex-preſs'd in the figure. In the middle it has a cylindrical cavity D Fig. 2. whoſe axis is exactly perpendicular to its inner and outer ſurfaces. Each of the boxes, in which the eye-glaſſes are contain'd, is fitted to this cavity; the above-mentioned braſs arm is fix'd into the inſide of this ſlider, towards the end fartheſt from the object metal; it riſes per-pendicular for about two inches, and is made flat, ſo as to turn one edge to the rays, which come from the object; about *b* it is bent forwards and flatted the other way; ſo that when the back of the oval plane is held flat to it, by the two ſcrews *c c*, the axis of the cylindrical cavity may fall on the center of its foreſide, inclin'd to its ſurface in an angle of ſomething leſs than 45 degrees: This angle is brought to be exact by two very ſmall ſcrews *i i*, whoſe threads take hold in the flatted end of the braſs arm, and their points bearing againſt the back of the oval, raiſe one end of it a little from the flat of the arm. The *ſpecula* are ſet at their

due


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A
Fig. VI
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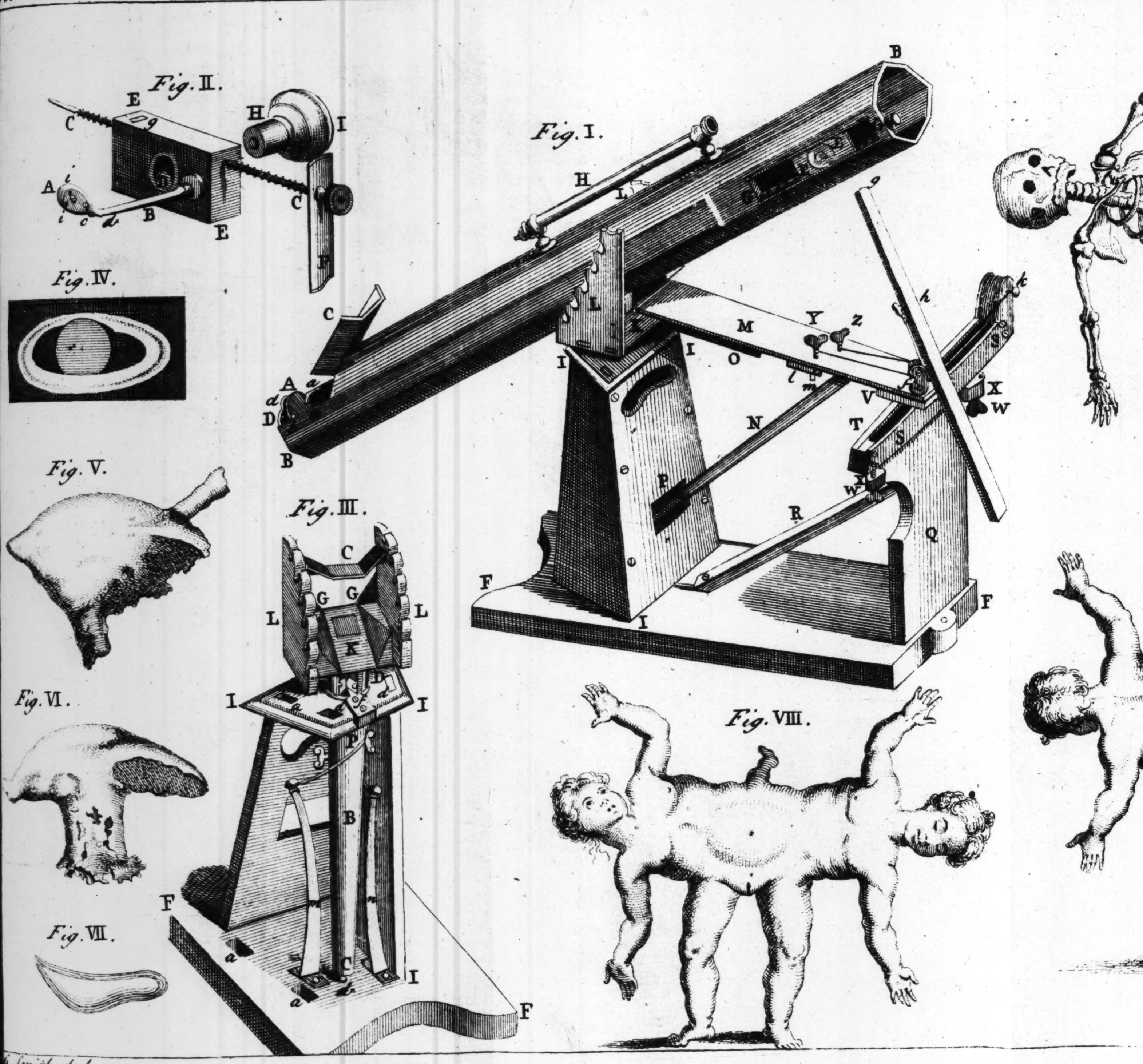

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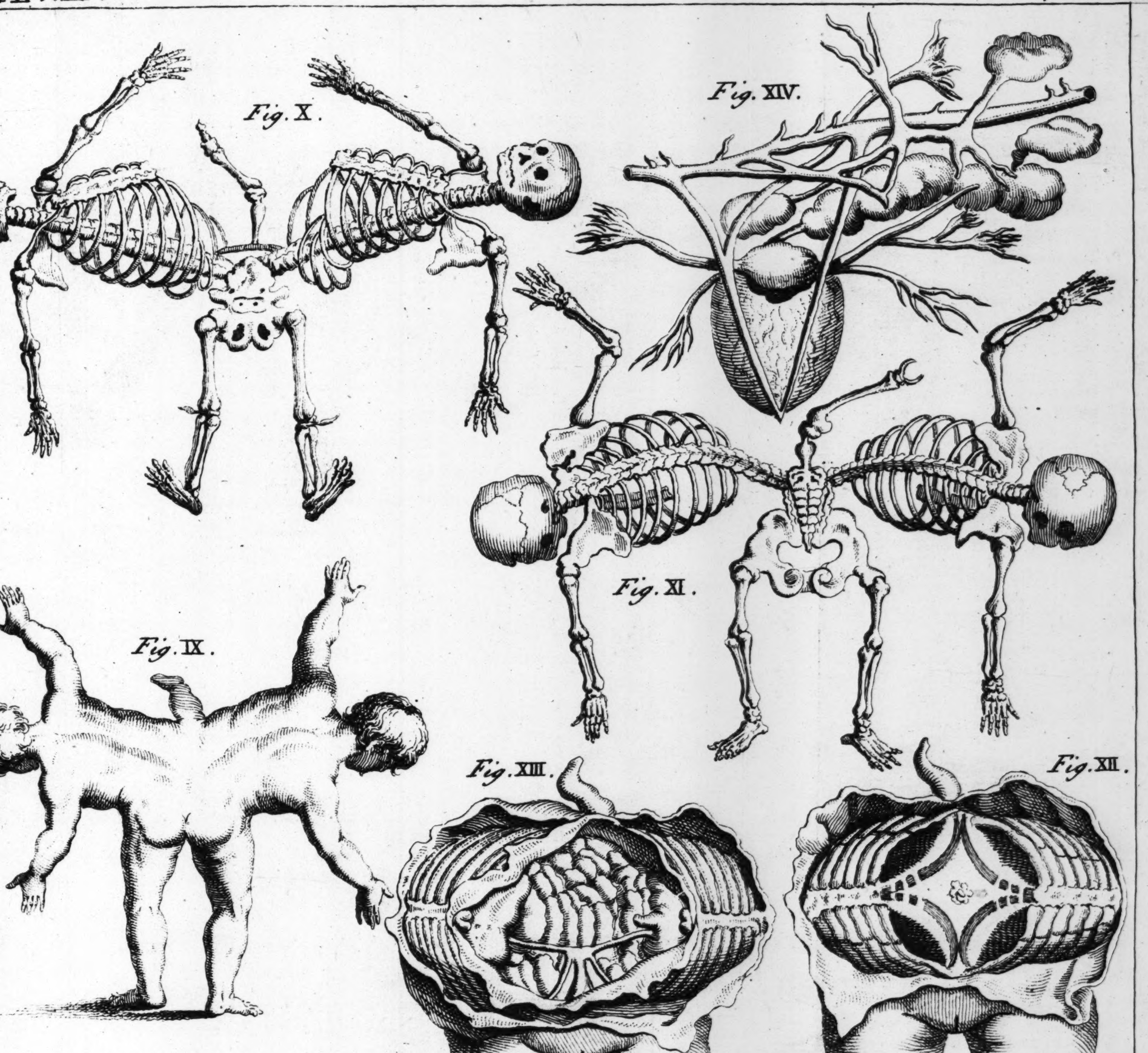
Fig. X.
Fig. XIV.
Fig. XI.
Fig. IX.
Fig. XIII.
Fig. XII.
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due diſtance, by turning a long ſcrew C C, for which there is a nut, lodged in the ſlider at *g*; the ſcrew is kept from moving backwards or forwards when it is turn'd, by a braſs plate F, which is to be fix'd to the flat end of the ſide of the tube, and taken off at pleaſure. Each of the eye-glaſs boxes H has a ſcrew on the outer end, to faſten it to a bowl or diſh I, to receive the ball of the eye, and guard it from external light.

On the top of the tube is fix'd on two ſmall pedeſtals, a common dioptric teleſcope, H Fig. 1. about 18 inches long, its axis parallel to that of the tube; and having two hairs, placed in the common *focus* of its object and eye-glaſſes, croſſing each other in its axis.

There are three convex eye-glaſſes, belonging to the inſtrument; the firſt or ſhalloweſt has its focal diſtance of about $\frac{1}{3}$ of an inch; the ſecond of $\frac{1}{10}$, and the deepeſt of $\frac{1}{40}$ or ſomething leſs. When the firſt of theſe is us'd with the inſtrument, it magnifies about 188 or 190 times in diameter; with the ſecond about 208, and with the third 228 or 230. Each of theſe glaſſes has a circle, placed in that *focus* neareſt the oval, to determine the part of the object, ſeen at one view; and in the other *focus* towards the eye, a braſs plate with a little hole in the middle, to let no light paſs to the eye from the inſide of the tube, but what comes from the oval. Beſides theſe three convex, there are two concave eye-glaſſes, with which it magnifies about 200 and 220 times, and alſo a ſet of three convex, which turn it into a day-teleſcope, magnifying about 125 times: The aperture is limited by a circle of card, or paſteboard, placed before the object-metal in the tube. To vary the aperture there are three of theſe circles, and the apertures allow'd by them are five inches and $\frac{1}{2}$, five inches and four inches and $\frac{1}{2}$; tho' for ſome objects the whole metal may be left open.

The engine, made uſe of to direct the tube to any object, conſiſts of a ſtrong plank F F, Fig. 1 and 3, about 14 inches wide and two foot and $\frac{1}{2}$ or three foot long, which ſerves as a foundation for the whole. Near one end of this plank is placed an upright four-ſided box III Fig. 1, 3, about two foot high, narrower at the back next the end of the plank than before: Its two ſides are mortis'd both into the plank below, *a a* Fig. 3. and into the top of the box above, *d d*; the back and forepart are faſtened to the edges of the ſides with wooden ſcrews; the top has a circular hole cut in it, ſomething above three inches in diameter, whoſe center is

about three inches diſtant from the outſide of the back, and at an equal diſtance from the two ſides; this hole gives paſſage to a turning pillar B, in the bottom of which is fix'd an iron pivot *c*, to turn in a thick braſs plate, lodged in the plank *b*. The upper end of the pillar riſes about an inch and a half above the top of the box, and is mortis'd into a ſtrong head K Fig. 1, 3. about eight inches in length, and four or five in breadth and thickneſs. This head carries two cheeks L L, about 13 or 14 inches in height, their hinder edges towards the lower end, extending five inches beyond the axis of the pillar backwards: Along the back of theſe cheeks, at equal diſtances above each other, there are notches tending obliquely downwards, and anſwering one another in each cheek, to receive the pivots of a crooked iron axis C, Fig. 3. on which the tube is placed. The notches are made at different heights, to keep the eye-glaſs at a proper height for the eye, in different elevations of the object above the horizon. The figure of the axis anſwers that of the three under ſides of the tube. The axis of the tube lies about two inches and a half higher than the axis of the motion upon theſe pivots; and the center of gravity, when the object metal is in, is about three inches more backwards. To keep the tube from ſlipping back, when its fore-end is rais'd, it has two buttons fix'd to it, that reſt againſt the fore-part of the axis.

To keep the pillar from touching any of the ſides of the round hole, in which it turns, a cylindrical ſector, containing about 65° or 70°, and about an inch in height, is cut out on the back part of the pillar, near the upper end D; in the angle of this cavity, is fix'd a thin ſteel plate *o o*, bent croſs the middle to the ſame angle; the internal angular edge, between the two parts of this plate, lies in the axis of the pillar, and turns upon the hardened edge of a wedge-like iron *f*, whoſe baſe, or board part, is faſtened with two ſtrong ſcrews on the top of the box, directly behind the round hole above-mentioned. The upper parts of the cheeks are ſtrengthened by two brackets G G, leaving room between them for the bottom of the tube to touch the upper edge of the fore-part of the head; the hinder part of the head is alſo hollow'd in the manner repreſented in Fig. 3. The head on its fore-part carries a flat arm M, Fig. 1. about 27 inches long, ſomewhat tapering towards the farther end, where it is four inches broad: This is ſtrengthened by a narrow ſlip, glu'd edgeways along the middle underneath O, and alſo by a brace

of

or ſtay N, reaching from the turning pillar to within nine inches of the end of the arm. The ſtay paſſes thro' a tranſverſe opening, cut in the fore-part of thebox P, which is long enough to allow room for a ſufficient motion of the pillar round its axis.

On the other end of the bottom plank, a-croſs its length, is erected a board Q about 12 inches wide, and 26 or 27 high, the top of it reaching within an inch and an half of the under ſide of the arm: This board is held firm in its poſition by a ſpur R; part of its upper end on the outſide is pared off towards the edges, to form it into the ſegment of a cylinder, whoſe axis coincides with that of the pillar; its uſe is to ſupport a reſt S S, on which the end of the flat arm moves backwards and forwards: This reſt being applied tranſverſely to the outer part of the upright board, where it is made cylindrical, is bent into the ſame figure, by the means of four ſcrew-pins, two of which paſſing thro' each end of this and of another piece of the ſame length, T (but ſomewhat narrower) placed over againſt it on the inſide of the board, by their nuts, draw them together, ſo as to graſp the end of the upright board between them; the upper edge of the reſt being firſt ſhot with a plane, very ſtrait and ſmooth: To render the motion of the arm along the reſt ſmooth and eaſy, it has two rollers, lodged in a box, fix'd near the end, on its under ſide V, to roll upon the edge of the reſt, when the end of the arm is mov'd along it; one of the rollers is placed near each edge of the arm, and their axes lie in lines, paſſing thro' the axis of the turning pillar; the reſt is kept up to them, with a proper degree of force, by two ſcrews W W, which run into two plugs X X, faſtened on the ſides of the upright board, and bear againſt the under ſides of two pieces, fix'd on the inſide of the reſt.

The motion of the tube is directed by two braſs pegs Y and Z; the firſt of theſe Y is placed about 10 or 11 inches from the end of the arm, and has a line wound round it, which paſſing under a ſmall pulley *f*, fix'd in a vertical poſition, near the end of the arm, is faſtened to a ſtaple on the underſide of the tube *g*: This line, by the turning of the peg, brings the fore-end of the tube to its due elevation, being acted againſt by the exceſs of weight in the hinder end of the tube, when the metal is in it, which is equivalent to about two pounds at *g*, where the line is faſtened. In great elevations of the object above the horizon, the line is not carried ſo far, as the point *g*; but is faſtened a little above the pulley, to

a light ſquare ſtick *h*, having a hook at one end, by which it takes hold of the ſtaple *g*. This is done, that the ſpringineſs of the line may not continue a vibrating motion in the tube (when any thing happens to ſhake the inſtrument) and make the object appear to tremble; the lower part of the ſtick reſts againſt the end of the arm, and by its ſlight friction contributes to the ſame effect.

The other peg Z is placed in ſuch a manner, that it may be conveniently reach'd by one hand of the obſerver, while the other is employ'd about the peg Y; it regulates the horizontal motion of the tube, by means of a line, which being wound about the peg at one end, paſſes by another ſmall pulley, placed cloſe by the ſide of the above-mentioned one in an horizontal poſition (not to be ſeen in the figure) and is hung on a pin, driven into the little head K; it is acted againſt by two ſprings *m* and *n*, Fig. 3. placed in the box III, one on each ſide of the turning pillar; that on the right hand *m*, draws the right ſide of the pillar forwards, by a very ſtrong line, which, being faſtened to the head of the ſpring, paſſes round the back part of the pillar to a pin at P, by which it is ſtrain'd to its due ſtrength: The ſpring on the left hand *n* draws the left ſide of the pillar backwards in the ſame manner. Theſe pins are placed on the pillar a little higher than the tops of the ſprings, that being drawn a little downwards, as well as turn'd round its axis, the pivot in its bottom may not be rais'd out of the hole in the braſs plate, when the reſt bears hard againſt the rollers at the end of the arm: Each of theſe ſprings draws with a force, equal to about 18 or 20 pounds weight, when the end of the arm is carried cloſe to the ſmall head *k* Fig. 1. and conſequently (the ſemi-diameter of the pillar being 1 inch and ½, and the diſtance of that head from the axis about 28 or 29 inches) the end of the arm will be carried by the united forces of both the ſprings, towards the other end of the reſt, with a force equivalent to the weight of about two pounds; each of the pegs Y and Z turns in a hole, made in a piece of wood *l*, faſtened to the under ſide of the arm; and the pieces being ſlit with a ſaw from one end thro' the hole, and about ½ an inch beyond it, the ſeparated parts are drawn together by a ſcrew *m*; till the end of the peg be griped between them, with a due degree of force: By theſe pegs, with the help of the teleſcope H, the tube is eaſily directed to any object, and made to accompany a celeſtial one in its diurnal motion; while the end of the arm moves the whole length of the reſt.

If it be desir'd, that when the object is found, the turning of one peg shall carry the tube along with the motion of the heavens, so as to keep the object always in sight; this may be easily effected in various manners.

The concave surface of the object metal has several little spots in it, which could not be brought to take a polish: In one or two places, the metal itself seems to have some small parts, something harder or softer than the rest, occasioning an irregularity in the figure of the metal about them. But these parts being small, in proportion to the whole, do not seem considerably to affect the distinctness of the appearance.

The open air has commonly an undulating motion in its parts, especially in the day-time, which occasions the rays of light to deflect a little from the streight lines in which they ought to move, in order to render the species perfectly distinct. The effect of this, tho' insensible to the naked eye, or even thro' a small telescope, becomes considerable, when the object is very much magnified. This instrument, when tried at an object inclos'd, so as to secure it from this inconveniency, seems to bear an aperture of 5 inches and $\frac{1}{2}$, with the deepest of the above-mentioned eye-glasses, as well as the common telescopes do the usual charge and aperture given to them, excepting that in these the objects appear a little brighter.

Fig. 1. represents the instrument, placed on the machine in order to be applied to use.

Fig. 2. represents the inside of the slider, with the rest of the *apparatus*, belonging to the oval plane and eye-glass.

Fig. 3. represents the hinder part of the machine; the back and one side of the box being taken away, to shew the turning pillar and springs on the inside.

Fig. 4. represents *Saturn*, as he appear'd in *June* 1720, by this telescope.

The Dissection of a decrepid old Man, 109 Years of Age; by Dr. John James Scheuchzer. Phil. Trans. N° 376. p. 313. *Translated from the* Latin.

THE different stages of human life have their own peculiar characters: For, man gradually ascends from a state of fluidity thro' the various degrees of solidity, till at length he die with a rigidity and immoveable stiffness of the

the fibres, as it were; the consideration of this is of no mean use in physick.

John Leonardus Vopper, a *Grison*, was born *May* 1. 1614. In 1634 being employ'd as a miner, he lay buried in a vein under ground for 33 hours; and being taken out from thence, he ever after, by reason of the strong pressure on the *Abdomen*, was subject to incontinency of urine. In 1637 he travell'd thro' *Hungary*, *Turky*, and the *Holy Land* and return'd to *Venice*: In 1639 he serv'd in the Duke of *Lorrain's* army in *Milan*, and in 1662 in *Portugal*: In 1682 he was at the siege of *Vienna*, and in the last war, at the siege of *Landau* and battle of *Hockstet*. Thus thro' a variety of scenes he liv'd till he was 109 years and three months old.

In a cursory view of his body, the following particulars were observ'd.

There was a small quantity of bloody extravasated *serum* in the cavity of the *abdomen*.

All the thin guts were inflam'd, and tinged with a red colour; and particularly the *duodenum* was very much dilated, its inside quite mortified and gangrenous; the *omentum* was entirely emaciated, so as hardly to be distinguished; the *pancreas* contracted; the liver sound; the gall-bladder full of bile; as also the *ductus choledochus*, and all the parts adjoining to the intestines and mesentery tinged green, as if the bile had been extravasated some where, but its exit into the *duodenum* could not be traced: Near the *pylorus* in the upper part of the stomach, there was a flatulent expansion, as it were, bigger than a walnut. The kidneys were sound; as also the spleen, whose external membrane had here and there bright white specks, some broader than others, which at first sight resembled the ripe pustules of the small pox, but entirely of a cartilaginous hardness, and somewhat rais'd above the rest of the surface. Here Dr. *Scheuchzer* observes, that the fibres do not only contract and become rigid in extreme old age, but almost insensible; for, no young person, or even one of more advanced years, could endure an inflammation of the intestines, such as this was, without the most exquisite pain; yet this patient did not in the least complain of it: That cartilaginous crust on the spleen plainly shews, that the membranes may become so rigid, as at length to become hard.

There

There was a great deal of difficulty in opening the *thorax*: For, not only these prominences of the *sternum*, into which the ribs are inserted, and which are otherwise cartilaginous, were in this subject plainly bony, but almost form'd one continued body with them; a manifest proof that cartilages in time become bony, as membranes and arteries first become cartilaginous, and afterwards bony: The lungs in both sides of the *thorax* had several green spots, and behind they adher'd to the ribs: The *pericardium* was large and fill'd with a great deal of *serum*, some of which floated in the cavity of the *thorax*: The heart itself was pretty large, particularly, the auricles were exceedingly dilated; and both they and the ventricles were full of concreted blood; whence it appears, that the motion of the blood was at length stopp'd, for want of the elastic contraction of the arteries, and that the machine of the heart was no longer able to throw out the blood, disposed to concretion; nay, that it was overpowered by the too great resistance: That tendon, by which the arteries are inserted into the heart, was either bony, or cartilaginous at least: It becomes bony, we know, in old deers, whence the bone in the heart of a deer: And some such thing was observ'd in this subject; for, the semilunar valves, particularly those of the *aorta* were almost cartilaginous, especially in the middle; the descending *aorta* was exceeding large, being double the diameter of the gullet, and about a *Paris* inch: The gullet had several indurated lentiform glands. This size of the *aorta* shews, that the progressive motion of the blood, probably, become slower in his latter years; whence the arteries, next the heart, were gradually dilated, as is frequently the case, when the resistance of the mass of blood to be thrown out is too great.

To come next to the head; the substance of the *cranium* was exceeding hard: The sutures, particularly the sagittal and lambdoidal, were almost obliterated, nor did they quite penetrate both tables, so that there was no perspiration; and the nutritious juice of the bones, extravasated between the fissures of the sutures was also become bony: It is true, there were three or four pretty large holes in the *vertex* on each side the *sinus*'s of the sagittal suture, about an inch from each other, that penetrated into the internal *lamina*, as far as the external, yet without any of them perforating both tables: The *dura mater* was near thrice its usual thickness, almost like a piece of leather; the *pia mater* was easily separated by being moif-

moiſtened with too much *ſerum*, from the ſubſtance of the brain: All the ventricles were full of *ſerum*, and a large quantity in the baſis of the brain, eſpecially at the exit of the ſpinal marrow. The *plexus choroides* was covered with glands of the bigneſs of a pea, and full of a coagulated *lympha*: The *ſeptum pellucidum* was very diſtinctly ſeen: Moreover, the ſubſtance of the brain, particularly the internal part, was more flaccid than uſual.

The Dr. was informed, after the death of this old man, that in his life time he had told perſons of credit, that his father, after having liv'd above a century begot three children; and that few of his anceſtors liv'd leſs than a 100 years: This is certain, that the old man here ſpoken of, had a daughter baptiz'd at *Dieſſenhove Aug*. 18. 1707.

An Account of the Coati Mondi *of* Braſil; *by Dr.* George Mackenzie. Phil. Tranſ. N° 377. p. 317.

THE *Coati Mondi* of *Braſil* is ſeldom or never brought alive into *Europe*; yet there were two of them found in Capt. *Green*'s ſhip, a pyrate; one of which died in Dr. *Mackenzie*'s cuſtody of a wound it had receiv'd in the thigh, which he cauſed to be diſſected; an account of which, compared with that, publiſhed by the Academiſts at *Paris*, the Dr. communicated to the *Royal Society*; it differed from theirs in ſeveral particulars, moſt of which might, as the Dr. thinks, proceed from the difference of ſex, theirs being a male and his a female.

Theirs was ſix inches and ½ from the end of the ſnout to the hinder part of the head; his was only four inches: Theirs was 16 inches from the *occiput* to the beginning of the tail; his was 10: Theirs was 13 inches from the inſertion to the end of the tail; his 12: Theirs was 10 inches from the top of the back to the extremity of the fore-feet; his was 7: Theirs was 12 inches from the top of the back to the extremity of the hinder feet; his 8: The ſnout of theirs was very long and moveable, like that of a hog, but ſtreighter and longer in proportion; his was only 2 inches: The fore-paws had each five toes, the claws of which were black, long and hollow, like thoſe of the *Caſtor*: The toes of the fore paws were ſomewhat longer than thoſe of the hinder-paws; the ſoles were without hair; the palms and ſoles of theſe fore-paws were covered with a ſoft and tender ſkin; the ſole of the hinder-paw was long, having a heel, at the extremity of which were ſeveral ſcales a line broad, and five or ſix lines long; in all which they perfectly agreed.

The

The ears were round, like thoſe of a rat, and covered a-top with very ſhort hairs; and in this they likewiſe both agreed, as they did in the eyes, which were exceeding ſmall and beautiful; but there was ſome difference in the hair: For, theirs was ſhort, rough and knotty, blackiſh on the back and head, and the reſt of the body, mixt with black and red; but in his, the hair was long, in proportion to the animal, eſpecially on the tail, and the whole was beautified with white and black circles, which made it have a moſt lovely aſpect. But from the ſnout down all the throat and belly to the top of the tail and inſide of the legs, was of a reddiſh colour: The tongue both of theirs and his was chopt with ſeveral fiſſures or ſtrokes, which made it rough to the touch. The *inciſores* were ſix in each jaw: The *canini* were very large, eſpecially thoſe of the lower jaw; but they did not turn up like tuſks, as theirs did; they were not round, blunt or white, like thoſe of a wolf, dog, or lion, but ſharp, by means of three angles, which at the extremity form'd a point like an awl: As to their colour they were greyiſh and ſomewhat tranſparent: The *gula* was large and cleft like a hog's; and the lower jaw, as in a hog, was much ſhorter than the upper.

By the diſſection the Dr. found in this, as the Academiſts at *Paris* had done in theirs, that under the ſkin, and between the muſcles, there was a great deal of fat, white and hard, like tallow: Theirs, being a male, had a *penis*, provided with a bone, whoſe length did in proportion very much ſurpaſs that of the bones, which are found in the *penis* of other animals; ſo in his, being a female, he obſerved, that it had an exceeding large *matrix*, and that the inſertion of the *urethra* was upon the right ſide of the *vagina*: The *epiploon* in his, as in theirs, was very ſmall; it had but little fat, and was a complication of fibres and fillets, rather than a membrane; it did not lie upon the inteſtines, but touch'd upon the ſtomach. In theirs they obſerved a very large ſpleen, but in his he could diſcern none. He did not obſerve, any more than they had done, any veſſels in the external membrane of the ſtomach, but the *coronaria ſtomachica*, which appeared as in theirs, towards the upper orifice and ſoon diſappeared, ſhooting forth a few branches. The liver in his, like theirs, was ſomewhat blackiſh, and of a ſubſtance very homogenous, without any appearance of glands. It had ſeven lobes, two large ones on the left ſide, and five ſmall ones on the right. The *pancreas* in his, as in theirs, was faſtened along the *duodenum*, inclining more towards the right kidney

kidney than the left: But whereas it was very small in theirs, it was very large in his. The mesentery in his, as in theirs, was filled with a very hard fat, which inclosed and almost conceal'd all its vessels. The intestines in theirs were seven foot long and all of one thickness, having nothing to distinguish them; but in his they were only 42 inches and ½. Theirs had no *cæcum*; but he found one in his at the upper end of the *rectum*. The bladder was very large; the right kidney in his, as in theirs, was a great deal higher than the left, and covered with the lobes of the liver. The lungs in theirs had five lobes, two on the right side and two on the left, and the fifth in the *mediastinum*, which was as thin as a cob-web; but in his there were seven lobes, three on the right, three on the left and the seventh in the middle. The heart in his, as in theirs, resembled that of a dog, having the right auricle exceeding large; and as they found a great deal of slimy matter, hardened in the right ventricle; so he found in his a *polypus*. The *musculus Crotophites*, passing under the *zygoma*, was in his, as in theirs, fastened there, being extraordinary fleshy, even to its insertion, made by a very large tendon, which was inclosed between two pieces of flesh, much thicker than those, which are generally found in this place, and which are thought to be put there to defend and strengthen the tendon of the muscle of the temples.

The tendons in the articulations of the fore-feet were very large and strong: In his he observed two glands on each side of the *anus*, with a passage to each of them, full of a greyish fetid matter. The orbit of the eye in his was not bony throughout like theirs, but supplied in the upper part by a cartilaginous ligament, which joined the *apophysis* of the *os frontis* to that of the first bone in the upper jaw. The bone, which separates the *cerebrum* from the *cerebellum* was, as it is in dogs. The *dura mater* in his did not adhere to the *cranium*, as in theirs. The *sinus*'s of the *os frontis* in his, as in theirs, were full of matter, like a friable fat. The mammillary *processes* in his, as in theirs, were very large. As to the eye, both of them agreed exactly, the globe not exceeding four lines and ½ in diameter, the aperture of the lids being much larger, and the pupil as large as the whole globe of the eye; the crystalline humour contained three lines in breadth, and two and ½ in thickness, and was more convex internally than externally; this thickness of the crystalline humour made the two others to be less in quantity. The *choroides* was all over of the same colour, *viz.* of a very brown red, without any

tapetum,

tapetum, which is hardly ever wanting in the eyes of other animals.

These are all the remarkable differences he could discover between his own and that of the Academists at *Paris*; only the Dr. gives some account as to the manner of its living and its diet, which they had not an opportunity of observing in theirs.

The Dr. believes the Academists were misinformed, when they affirm, that the *coati mondi* carry their tails erect; the tail of this at least trailing on the ground; nor can he think, that they eat their tails; for, there was no part of her she could endure less to be handled than her tail, the least touching of it would make her cry, or rather hiss like a snake; she could endure no manner of cold; for, in the intervals between the times of eating, she was either beneath the bedcloaths, or on a cushion before the fire, with the heat of which she seemed to be extremely well pleased.

Her ordinary food was butter'd eggs, milk and bread, all manner of roasted flesh, but no fish: The Dr. once tried her with a new kill'd partridge, of which she eat most voraciously; and, for several days after, she was very wild and ungovernable, which made him never after try her with raw flesh. He was likewise apt to think, that their ordinary dens or habitations are under ground in sandy banks, like our rabbits: For, when she was brought into the fields, she would dig up the sand with her paws, with incredible swiftness; so that had she not been chain'd, there would be no possibility of recovering her.

An Account of a large Quantity of Stones *voided by Drinking the* Pyrmont Waters; *by Dr.* Abraham Vater. Phil. Trans. N° 377. p. 322. *Translated from the* Latin.

A Nobleman of *Pomerania*, in perfect health (as he himself affirmed) and without any sensible ailment, was persuaded to drink, by way of preservative, the *Pyrmont waters*: After drinking them for some days, when he made water he felt some small stones come away, without any pain: Judging this to be critical, he began to drink the waters in a larger quantity; upon which, in four or five days after, he voided upwards of 40 stones, yet still without any pain: Upon observing this, he continued to drink them chearfully and plentifully, promising himself a great deal of good thereby. But by drinking too large a quantity of the waters, he

he had a total suppression of urine for three days, till by a catheter a passage was opened: He, therefore, by the advice of his physicians, abstained from any farther use of the waters, supposing the danger thus over. But not long after, being on a journey, he had a constant desire of making water, till at length he voided pure blood with the most exquisite pain: This painful voiding of blood continued all the time of the journey: but after his return home, and that he was quiet, it immediately ceased and he felt no more pain: But ever after as often as he rode either in a chariot or a horseback, his pain returned, and he voided blood with his urine; but when he was quiet, it again ceased without the least pain: It is true, it sometimes happened, that he voided a stone or two, but that was rarely and without any pain; because they were much smaller than those voided by drinking the waters. The patient had this troublesome symptom upon him for two years, but at length was relieved by the use of medicines. And now being apprehensive of nothing more, and seemingly restored to perfect health, he forbore the use of them: But from that time his strangury daily increased, and he had a constant desire to make water, which still continued upon him. His urine, which was voided in a very small quantity, and with very great pain, was thick, from the admixture of a viscid *pituita*, that immediately subsided and resembled paste, and could be drawn out into threads; the patient had all the time a good appetite, slept pretty well, and felt no pain in any other part.

This case, considered in all its circumstances, the Dr. thinks, has scarce a parallel in medical observations. What is most remarkable is, that a man, who (as he himself affirm'd) was in perfect health, and who never had any signs of the stone, should, by drinking these mineral waters, void pretty large stones, in such quantities, and in so short a time. This may give no small reason to suspect, that these stones were not discharged by the waters, but rather generated from the mineral *crocus*, with which these medicinal waters are impregnated. Tho' the very short time in which this happened (scarce sufficient for the concretion of so many stones) seems little to favour this opinion; because voided a few days after the patient began to drink the waters; yet he thinks it not easy to determine, what time is requisite for the production of a human *calculus*: which probably may be very short: And it seems less agreeable to truth, that these

calculi

calculi ſhould lie concealed in the urinary paſſages; ſince this patient never had any difficulty in making water nor the leaſt ſymptom of the ſtone: On the contrary, all the circumſtances of this caſe confirm the concretion to be inſtantaneous: For, the ſtones had no aſperities and angles, as thoſe of the bladder commonly have, from the ſucceſſive coheſion of the ſaline *ſpicula*; but reſembled large and ſmall grains of peaſe, round, ſmooth and even, which ſhews their concretion to be ſudden: Their dirty colour exactly agreed with that ſediment, extracted from boiling the *Pyrmont-waters*: Upon breaking the *calculi*, the ſame variegated *ſtrata* were obſervable in the inſide, as in the *tophi* of the *Caroline-waters*: What is peculiarly remarkable is, that the greateſt part of theſe *calculi* was voided at the time of drinking the waters, but afterwards very rarely, and theſe much ſmaller. Yet this extraordinary effect is not to be imputed to theſe medicinal waters, as if they contributed to the production of the ſtone, but rather to the weakneſs of the *viſcera* and to a bad digeſtion.

Obſervations on Diſſecting the Body of a Perſon, troubled with the Stone; *by Dr.* Perrot Williams. Phil. Tranſ. N° 377. p. 326.

MR. *William Bowen* of *Haverford-weſt*, between 40 and 50 years of age, having been, for about 7 years, ſeverely afflicted with the uſual ſymptoms of the ſtone in the kidneys and bladder, *viz.* bloody urine after exerciſe, ſtrangury, &c. died in *May* 1722. Upon opening his body, there were found in the bladder ſix ſmooth oval ſtones, exactly of the ſame figure and nearly of the ſame bigneſs: There were likewiſe three cells in each kidney, the figure of each anſwering that of the ſtones: The *ureters* were ſo preternaturally extended, as very eaſily to admit the largeſt of the ſtones to paſs from the kidney to the bladder. The *viſcera*, &c. appeared in their natural ſtate.

The Diſſection of the Body of a Man, *who Died of the Stone in the* Kidneys; *by Dr.* Hardiſway. Phil. Tranſ. N° 377. p. 327. *Tranſlated from the* Latin.

A Man about 70 years of age, was *October* 19. 1722, ſeiſed with a very violent colic, attended with vomiting and a ſtrangury, without being able to ſtand upright, by reaſon of the exquiſite agony he was in: On the 6th day of his illneſs,

illness, after a total suppression of urine, he complained of a painful constriction about the *hypocondria*, as if bound with ropes, and afterwards of a troublesome weight in the bladder, as if a large turnep (for, so he call'd it) were lodged therein. The symptoms growing worse and worse, the patient died on the 12th day of his illness. Upon opening the body, the Dr. cut the bladder, but found not the least sign of a stone therein; but in both kidneys (which contained a large quantity of water) he found a remarkable stone (represented Fig. 5 and 6. Pl. VIII.) rough, and spreading into a variety of branches thro' the *parenchyma*, and which could not possibly be taken out whole, without first cutting away the flesh; the largest branch, insinuating into the head of the *ureter*, shut it up entirely: The *omentum* was retracted under the liver, like a narrow belt. The patient was of a very robust constitution and always healthful; only a few years before, he sometimes voided gravel with his urine.

Fig. 5. A represents the upper part of the stone which lay in the left kidney; B its *apex* towards the ribs; C a branch; D the branch, that shut up the head of the *ureter*.

Fig. 6. *a* represents the *apex* of the stone (which lay in the right kidney) regarding the ribs; *b* the upper part; *c* a protuberance; *d* a branch extending towards the *coxa*; *e* a cavity, where an officle was broken off, forming a protuberance or process; *fff* the lower part of the stone, that possessed the head of the *ureter*.

An Account of the Depth of Rain *fallen from* April 1. 1722 *to* April 1. 1723. *observed at* Widdrington *in* Northumberland; *by Mr.* Horsley. Phil. Trans. N° 377. p. 328.

MR. *Horsley* kept an exact account of what rain fell at *Widdrington* in 1722: Weighing the water and reducing it from weight to depth seemed pretty troublesome, even when done in the easiest method: To remedy this inconvenience (besides a funnel and proper receptacle for the rain) he made use of a cylindrical measure and gage. The funnel was 30 inches diameter, and the cylindrical measure exactly three inches; the depth of the measure 10 inches, and the gage of the same length, with each inch, divided into 10 equal parts; or instead of a gage, the inches and divisions may be mark'd on the side of the cylindrical measure: The *apparatus* is simple and plain, and it is easy to comprehend the design and reason of the contrivance. For, the diameter of the cylindrical measure being just $\frac{1}{10}$ of that of the funnel, and the measure exactly 10 inches deep

deep, it is plain, that 10 measures of rain make an inch in depth; 1 measure, $\frac{1}{10}$; 1 inch on the gage $\frac{1}{100}$, and $\frac{1}{10}$ of an inch on the gage, $\frac{1}{1000}$, &c. By this means the depth of any particular quantity, which falls, may be set down with ease and exactness; and the whole at the end of each month of every year may be summ'd up without any trouble.

By the following account it appears, that some of the summer months, particularly *May* and *July*, were very wet, and some of the winter ones very dry; so that one with another, this year's rain, as far as can well be conjectured, may be reckoned as a medium; and if so, it differs not above two or three inches, from the mean quantity of rain which falls at *Upminster*, *Paris* and *Lisle*; being less than that at *Lisle* and more than that at the other two places.

An Account of the depth of rain fallen from *April* 1, 1722 to *April* 1, 1723.

	Inch.	Ten.
In *April*	1	, 015
In *May*	3	, 532
In *June*	2	, 570
In *July*	4	, 350
In *August*	2	, 132
In *September*	1	, 155
In *October*		, 600
In *November*	2	, 205
In *December*	1	, 780
In *January*	1	, 225
In *February*		, 485
In *March*		, 195
In the whole year	21	, 244

Of the Blood-globules, &c. *by M.* Leewenhoeck. Phil. Transf. N° 377. p. 341. *Translated from the* Latin.

DR. *Jurin* having discovered a method of determining with certainty the diameters of minute objects; and consequently, that the diameter of a blood-globule was equal in bulk to

to the $\frac{1}{1940}$ part of an inch, M. *Leewenhoeck* reasoned in thi[s] manner, if the diameters of 1940 blood-globules be equal i[n] length to 1 inch; moreover, if spheres be to each other, as th[e] cubes of their diameters, as Geometricians demonstrate; it fol[-] lows that a body, consisting of 7,301,384,000 globules, is n[ot] bigger than a sphere, whose axis is equal to 1 inch in length.

M. *Leewenhoeck*, upon viewing a-fresh his own blood an[d] that of two other persons besides, plainly discovered, that fou[r] diameters of blood globules were, according to Dr. *Juri[n]* equal to the diameter of a fine silver wire, which the Dr. ha[d] sent the former; 485 diameters of this silver wire were equal t[o] 1 inch or $\frac{1}{12}$ part of an *English* foot, as found by the method exhibited in *Phil. Trans.* N° 355. M. *Leewenhoeck* only add[s] that he could observe with his naked eye (some tho' very few blood-globules, four diameters of which somewhat exceede[d] that of the silver wire: But he imagined, that these globule[s] drawn from his thumb by pricking it with a needle, and lai[d] upon a glass-plate, whilst they were still warm, had deviate[d] somewhat from a round to a plane figure.

Moreover, to this he adds, that besides his sight being muc[h] impaired by his extreme old age, his right eye was somewha[t] dim; which he supposes to be owing to a great many blood globules floating in the crystalline humour before his sight some of which being joined together in no certain order, an[d] others floating a-part, occasioned a kind of *nubecula* in his eye For commonly using his right eye, when he viewed objects, h[e] could easily shut his left; hence it seemed not to be so clea[r] sighted as usual.

In *January* 1723, M. *Leewenhoeck* was seised with a violen[t] motion about the diaphragm, at which the by-standers wer[e] very much surprised: Upon the remitting of that motion, h[e] asked a physician present, what he called that disorder, who tol[d] him it was a palpitation of the heart: But M. *Leewenhoec[k]* believed the physician was mistaken; for tho' during the mo[-] tion, he himself often felt his pulse, yet he could perceive n[o] acceleration thereof: That violent motion, recurring at time[s] lasted about three days, at which time his stomach and intestine[s] lost their motion, and did not perform their office; upo[n] this M. *Leewenhoeck* verily believed he was near his end.

He supposed, that the obstruction in his diaphragm, was as big as a *Rix* dollar.

M. *Leewenhoeck* looks upon the common opinion of generation from the *ovarium* as absurd: For several years together he examined the *semen masculinum* not only of birds and fish, but likewise of other animals; and this he chiefly did every year, when fish spawn: And he commonly observed, the *semen masculinum* abounding in a vast number of very minute *animalcula*, of which it chiefly consists: Whence he certainly concluded, that these *animalcula* gradually become animals of the same species, with those they sprung from; excepting eels, shrimps, shell-fish and several sorts of flies, which are all females and which in this respect agree with vegetables; in whose seeds we find little plants, which instead of the *semen masculinum* again produce fruit of the same kind with that from which the seeds sprung.

Some Amendments and Additions to the Account of Things found under Ground in Lincolnshire; *by Mr.* Ralph Thoresby. Phil. Transf. N° 377. p. 344.

WHereas, in *Phil. Transf.* N° 279. there is some difference between the accounts of the depth of the things, found in *Lincolnshire*; the one reckoning it to be about 8 or 10 foot deep, the other 12 or 14: It is to be noted, that the depth was not measured, but conjectur'd and accounted for according to the relator's best remembrance. But the difference may be easily accommodated or salv'd, by supposing (which will not be far from truth) that when the labourers or dykers first discover'd the jetties and other things found therein, it might be about the depth of 8 or 10 foot; but the bottom of them, when they came to be all taken up, might be at the depth of about 12 or 14 foot, as in the other relation.

It is also to be noted in the said *Transaction*, that some very judicious persons affirm, that the stones, which the spectators saw at the bottom of *Hammon-beck*, were such, as the dykers had first thrown out (when they were taking up the old *goat*) and were fallen in again: But that it was a hard firm soil is certain; and probably, that on which the famous steeple of *Boston* stands; as to the foundation of the said steeple; vide *Phil. Transf.* N° 223.

The form of the shoe soles, found at *Spalding*, was as represented Fig. 7. Plate VIII. each foot had its proper shoe; this was for the right foot.

Now, by ſome paſſages in hiſtory, it may be probably co[n-]
jectured, when thoſe ſhoe ſoles were left there, and how lon[g]
ſince, that alteration began in that part of *Lincolnſhire*.

In *Stow*'s chronicle *ad An.* 1465 we read of a proclamatio[n]
againſt the beaks or pikes of ſhoone or boots, that they ſhoul[d]
not exceed two inches, upon penalties there mentioned: An[d]
by other paſſages in hiſtory it appears, that thoſe pikes of ſho[es]
were before that time exceeding long, and held up by chain[s]
that they might not hinder the wearer's walking; which chai[ns]
were ſometimes of ſilver, if not of gold, that they might b[e]
rich, as well as ornamental.

In *Melchior Adamus*'s life of *Conrad Pellican*, at the botto[m]
of p. 263. *Octavo Edit.* there is a paſſage to this purpoſe, '[A]
'that time, *viz.* in 1484, the ſoldiers, who returned fro[m]
'*Flanders*, introduced ſeveral novelties, to wit, party-colour['d]
'ſtockins, ſquare-toed ſhoes, which both men and women be[-]
'fore that time wore piked or ſharp pointed; as alſo a ne[w]
'kind of ſquare-toed ſandals, called **Pantoffeln**, which ſuper[-]
'ſeded the uſe of wooden ſhoes, called **Holtzſchuh**: In th[e]
'mean time theſe novelties became very faſhionable.'

Now by this account it appears, that it is not much abov[e]
200 years, ſince thoſe ſhoes, above deſcribed, were worn; an[d]
conſequently, it cannot be much longer, ſince the earth hat[h]
been raiſed there to the above-mentioned thickneſs, *viz.* 10 o[r]
12 foot; and ſince *Bicker-haven* grew up to be (as it is at pre[-]
ſent) higher land than the country on each ſide of it, it may b[e]
conjectured, what a change a century or two more may make i[n]
the out-falls of the rivers of *Witham*, *Welland*, *Nyne* an[d]
Ouſe; and conſequently, the neceſſity of taking ſome othe[r]
method for preventing the impending miſchiefs, which threate[n]
the navigation of the ſaid rivers, and thoſe who have eſtate[s]
and intereſts in the great level of the fens, and are concerned i[n]
the draining of them.

An Account of a monſtrous double Birth *in* Lorrain; *b[y]* *M.* Fevry. Phil. Tranſ. N° 377. p. 346.

ON the laſt of *December* 1722 M. *Fevry*, one of the Duk[e]
of *Lorrain*'s ſurgeons in ordinary, went by his orders t[o]
Domp Remy la Pucelle to ſee one *Sebaſtiana Camus*, 44 year[s]
of age, delivered on the 24th of the ſaid month, about eigh[t]
o'clock in the evening, of two children, join'd together in th[e]
manner, repreſented Fig. 8, 9. Plate VIII. There was one head
one neck, one breaſt, one *abdomen* and two hands on one ſide
and

and as many parts on the other side; the whole being well proportioned and plump, joined together by the belly, which was common to both; so that one of the heads was in the place, where the other's feet should be, and the other head in its natural place; they had but two legs for both of them, which seemed to arise from the transverse *apophyses* of the *vertebræ* of the loins on one side; and from the opposite region of the loins, came out a leg, ending with a joint, which bended forwards, and at the extremity formed a small stump, like a finger, articulated by *ginglymus*. There was but one fundament for both, by which they voided their *fæces*; they had but one navel-string, and the parts proper to the female sex were also single: They eat and drank with their mouths severally, and while the breast was given to one, the other cried for it: They slept and waked, sometimes both at the same time, and sometimes, separately. Each of these children had been baptised; one of them was somewhat plumper than the other, which was more puny and not so fresh-coloured. The head of the one, which was a little bigger than that of the other, came first to the birth, the two arms, lying on the breast came next; the legs lay on the sides of the breast of the second; the opposite leg, which was single, was extricated afterwards; last of all, the arms of the second child, being ranged on the side of its head, made it easy for the rest to come out. The bodies of both together were no bigger than that of an ordinary child.

It is observable, that the mother could assign nothing that had any relation to this event, during the time of her pregnancy.

Jan. 21. 1723. N. S. M. *Fevry* was informed, that both these children were equally well in health.

By another account, communicated to the *Royal Society*, these children liv'd two months after the birth.

Fig. 8. Represents the fore-view of the double child.

Fig. 9. The back-part.

Fig. 10, 11. The skeleton.

Fig. 12. The breasts and abdominal muscles, after the external integuments were removed.

Fig. 13. Part of the *viscera* of the *abdomen*.

Fig. 14. The urinary and genital parts.

The two first Fig. were drawn in their life time and the five following were taken after their death, by order of the Duke of *Lorrain*.

Observations and Experiments on the Sal catharticum amarum, *commonly called the* Epsom Salt; *by Mr.* John Brown. Phil. Transf. N° 377. p. 348.

THAT the salt, distinguished by the name of *Sal catharticum*, is made from what, at the salt works, is called *bittern*, is pretty commonly known; but the particular manner how this *bittern* is produced, and from it, these salts, has not hitherto been communicated to the world, in such a manner, as to become intelligible; and the *opprobrium* unjustly cast upon the salt, of its being a counterfeit *Epsom salt*, or something made in imitation thereof, or common salt dissolved and re-crystallised, has very much sunk its esteem among the learned in physic.

By the account M. *Bolduc* gives in the history of the *Royal Academy* of *Paris* for the year 1718, he did not succeed according to his wish in the experiments he tried, to find out what this salt was made from, justly grounding his reasons for its not being all made from the *Epsom* waters, or other springs that afford bitter purging waters, on the large quantities consumed and the cheapness of its price: After all his curious endeavours, it still remained a secret, till he received information from *England* by Dr. *Mendez*, whose account tho' very imperfect, is as follows: It comes, says he, from *Lemington* and *Portsea*, both in *Hampshire*, where from heaps of fossile salt there runs a saltish, bitter, sharp and pungent liquor. One would judge by its brackishness and bitterness, that it contained two sorts of salt, the one a sea-salt and the other a bitter salt. To separate these salts, they cause this liquor to run thro' hollow drains on the ground; there it gets together and condenses into salt; this they put into a large vessel, with a large quantity of common water and boil it as long as is sufficient to dissolve it; then they let it cool and settle for several days. The water, impregnated with the sea salt, which is the heaviest, sinks to the bottom of the vessel with the earthy parts, and the water impregnated with the bitter salt, which is the lightest, swims a-top. They take off this upper liquor, as long as it retains its bitter taste, without any pungency; afterwards they boil it in one or two waters, then evaporate it, and it yields white and transparent crystals, which are the counterfeit *Epsom* salt.

Dr. *Seipp* in his description of the *Pyrmont* waters p. 127 says, that the common *English* purging salt, which is sold in grea

great quantities in *Germany*, under the name of *Epſom* ſalt, is not prepared from the *Epſom* waters, but is made in *London* from common ſea ſalt and oil of vitriol.

In the ſame page he ſays, that the ſalt, obtained from the *Pyrmont* waters, will part with its own acid ſpirit, upon pouring oil of vitriol thereon, which the *Sal Mirabile* and the *Engliſh* purging ſalt will not. By this means he diſtinguiſhes the firſt ſalt from the two laſt.

Mr. *Quincey* in his *Prælectiones Pharmaceuticæ*, publiſhed ſince his death, ſays, there hath lately been contrived a ſalt from the mineral purging waters, made by evaporation, filtration and cryſtallization: It was firſt called *Sal mirabile* or *Sal catharticum amarum*; but it is now ſo ſcandalouſly counterfeited, that it is little elſe than common ſalt diſſolved and recryſtaliz'd.

Before Mr. *Brown* enters into the account, how this ſalt is made, he firſt ſays ſomething of the genuine ſalt, that has been made from the bitter purging waters, which the learned Dr. *Grew* attempted firſt to make at *Epſom*: Some years after, ſeveral other bitter purging ſprings were found in different counties, and ſalts in ſmall quantities were boiled up from them; but from no place, nor all the places put together, in ſuch large quantities, as from the ſprings on one ſide of *Shooter's-hill* in *Kent*, about the year 1700, which was then in the poſſeſſion of thoſe two ingenious chemiſts Mr. *George* and Mr. *Francis Moult*; and where they made ſuch large *apparatus* for evaporating the water, that they ſometimes boiled down 200 barrels in a week; from which in a dry ſeaſon, and when the land floods did not get into their drains, they obtained 224 pounds of ſalt.

After theſe works had gone on ſome time; Dr. *Hoy* found out a more expeditious way of making a purging ſalt, ſo nearly reſembling that from the purging ſprings, in all its properties, that it ſoon paſſed on the world for the other, and continued ſo to do.

The great conſumption of theſe ſalts (which then went only by the name of *Epſom* ſalts) both at home and abroad, engaged ſome of our phyſicians (ſeveral years before M. *Bolduc* took notice of it) to ſuſpect, that even what was made at *Shooters-hill* was ſpurious and receiv'd an addition of ſomething to increaſe the quantity: But theſe ſuſpicions, Mr. *Brown* poſitively affirms, were entirely groundleſs, as to the ſalts made there; tho' he readily believes the ſame of any

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other place, where the ſpring waters were boil'd down for ſalt: But upon a conſideration, that there were greater quantities of this ſalt, conſum'd than all the places, where the waters were boil'd could produce (which was the real fact at that time of day) there was ſufficient room to ſuſpect, that ſome of them were not genuine, as appear'd to be true ſome-time after.

For, the ſecret (which was then in a few hands) of making theſe ſalts cheap, gave thoſe who had it, an opportunity of underſelling thoſe who made it from the waters; and in a year or two render'd the latter incapable of making it to any advantage, conſidering the price it was ſold for by the former: So that the work on *Shooters-hill* was thrown up; and Mr. *Brown* believes, there has not been 100 pounds of ſalt made from the waters ſince that time, in any part of the kingdom.

Some time before this work at *Shooters-hill* was broke up, ſome pains was taken to diſcover the ſecret thoſe had, who ſold the ſalt ſo cheap; and upon examining the ſeveral ſalts, that were ſold about *London*, thoſe diſpos'd of by Mr. *George* and Mr. *Francis Moult* were certainly genuine, and were, therefore, a proper ſtandard to judge the reſt by. But from all the experiments made at that time, there could be no material difference found between the ſalt made from the waters, and that made by them who were in the ſecret: There was, it is true, a ſalt ſold by ſome, which, in the courſe of theſe trials, was found to be a *Sal-mirabile*, made from oil of vitriol and common ſalt, but ſhot into ſuch ſmall cryſtals, as not at firſt ſight to be diſtinguiſh'd from the other.

It was not long before this was diſcover'd, and the experiment was tried at the Lady *Carrington*'s ſalt works at *Portſmouth*; where it was found the ſame thing could be done, as at another work, not far from it, and in which Dr. *Hoy* had been concern'd.

It was ſome years after this ſalt had been made at *Portſmouth*, before the ſalt-makers at *Lemington* attempted, or indeed knew the method of making it; who now are the greateſt traders in it, and have ſent ſeveral tons in a year to *London*, beſides what has been directly exported from thence.

It was the opinion of the proprietors of the ſalterns near *Portſmouth*, that this purging ſalt could not be made at any other

other ſalt works but theirs, and that the bitter taſte in the ſalt was communicated from the earth to the ſea ſalt, whilſt it ſtood expos'd in their ſun-pans. But time has prov'd this opinion falſe; for, beſides what has been ſaid of its being made at *Lemington*, it was, about four or five years ago, *viz.* 1719 or 1720, begun to be made near *Newcaſtle*, where it is ſtill continued to be made; and doubtleſs may be made at any other ſalt-works, where the common ſalt is made from ſea-water by evaporation. Whether any thing of this kind has been attempted at any of our inland ſalt ſprings, either in *Cheſhire* or *Worceſterſhire*, Mr. *Brown* had not hitherto been ſatisfied.

There is ſome difference in the making the common ſalt in *Hampſhire*, from that about *Newcaſtle*: At the firſt of theſe places, in the beginning of the ſummer, at ſpring tides, or at new and full moon, the ſea-water is let into their feeding-ponds, which are their reſervoirs for their ſummer's working, and from hence is convey'd into ſmall ſquare pans; and ſome time after, it is again convey'd from theſe into larger pans or beds, which they call brine or ſun-pans; all which are made of ſea mud and the earth: In theſe laſt pans or beds, it lies expos'd to the ſun and wind, in order to exhale the weakeſt waters; and it is in theſe beds, if the weather prove very favourable, that they can make as good bay ſalt, as any we have from *France*, and at ſuch a time they never bring their brine to the boilers. But if the weather be not hot enough for that purpoſe, their brine is expos'd ſo long in theſe pans, till it become of ſuch a ſtrength, as to bear up their eggs, made of glaſs or wax, to a certain height above the ſurface of the brine; which from thence is convey'd into large ſtore ciſterns, and then into boiling pans, made of iron, where it is boil'd down (after having been frequently ſcumm'd) to the ſea ſalt. It is obſervable, that whilſt the brine is boiling, there precipitates a hard cruſty matter, which is partly taken out by veſſels, placed in proper parts of the pan for that purpoſe, and part of it fixes on the bottom of the iron pan ſo hard, as to be afterwards dug up; and this the workmen call *ſcratch*, and is what Dr. *Collins* in a former *Tranſaction*, concerning the ſea-water, boil'd at *Shields*, calls a ſtone powder. When the operation for the ſea-ſalt is finiſh'd, it is taken out hot, and put into wooden troughs with holes at the bottom, thro' which runs the ſuperfluous liquor: Under theſe troughs other veſſels are ſet (with ſticks fix'd in them

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in a perpendicular position) to receive what runs thro'. The liquor is suffer'd to continue some time in these vessels, and according to the quantity of the sea-salt still left therein, will crystallize to the sticks, somewhat like sugar-candy, but in much larger shoots; and this they call *cat-salts* or *salt-cats*, and it contains some share of bitter salt. When this salt is broke small, or rather powdered, it is so white, that some gentlemen choose it for their tables; but the greatest consumption of it is among the cake soap-boilers. The liquor that will not shoot to these sticks, is, what at these works they call the *bittern*, fit for making the *Sal catharticum*.

Near *Newcastle* their method is, to receive the sea-water into their reservoirs at high water, at any time of the moon, if there be no fresh water in the river, occasioned by rain in the higher country; and from these reservoirs, without exposing of it in beds, as at *Lemington*, they pump it into their boiling pans, where, evaporating it almost to a pellicle, they fill it up again eight or nine times, and then evaporate it with a gentle heat for the common or sea salt. The liquor that runs from this salt, when taken out and put into proper vessels, is what they call the *bittern*, from which, if it stands some time in those vessels, a salt will shoot and crystallize to the sides, in taste pretty much like sea-salt, but with some bitterness and seems to answer the *cat-salt* of the *Lemington* works, and, very probably, would shoot after the same manner, if they made use of the same *apparatus*.

Mr. *Brown* has given this general and loose account of making the common salt, as necessary to introduce the liquor call'd *bittern*; which, before Dr. *Hoy* found out an use for it was always flung away; being so different in its properties from the brine made use of to produce the sea-salt, that it would not boil up into a sea-salt again, and requir'd the nicest skill and attendance of the operator to determine the time, when to take out the sea-salt from the pans, before the bittern incorporated therewith, which would otherways spoil the whole making.

The *bittern* at *Lemington* (as observ'd above) not shooting to the sticks, is convey'd by channels into pits, made tight with clay, where it stands for some months, and there it will shoot again: What liquor remains is boil'd down, till such time as it is observ'd to be in a disposition to crystallize, and then it is convey'd into wooden coolers, lin'd with lead: The liquor, which will not shoot there, is boil'd down after the

ſame manner, in order for another cryſtalization. By this time the liquor ſeems to have alter'd its property, and becomes of a very pungent taſte, and if boil'd down, will no longer ſhoot into cryſtals as before, but precipitate, during the boiling, a ſmall grain'd ſalt; and if, for experiment ſake, you ſhould continue to boil down the liquor, ſeparated from this ſalt, each quantity of ſalt thus produced, will ſtill be more pungent than the other. If you boil down the whole quantity of this liquor, it will produce a ſalt, which, if expos'd to the air, will run *per deliquium*. The liquor, that produces this ſalt, is always flung away, wherever the *Sal-catharticum* is made.

This Mr. *Brown* can give no other name to than a third ſalt, produced from the ſea-water, differing in ſome reſpects, as much from the other two, as they do from each other.

To return to the ſeveral cryſtalizations, ſaid to be ſhot from the *bittern*; theſe will be of different ſizes, as to their figures, and contain ſome ſhare of the third ſalt juſt now mentioned, which makes them apt to give and diſſolve; nor is their taſte come yet to that ſimple bitter of the pure ſalt: Theſe, therefore, are either ſeparately or altogether, to be flung into a copper, with as much common water, as is ſufficient to diſſolve them, and allow of a gentle evaporation, till they are again ready to be pour'd into coolers, in order for cryſtalization. This generally proves to be the pure *Sal-catharticum*, throughly freed (as far as the experiments he tried could convince him) either from a ſea ſalt, or the above mentioned third ſalt. The liquor, decanted from this ſhooting, may be boil'd down again, in order for a ſecond ſhooting, and after that a third: But as the liquors from theſe ſhootings are boil'd away more or leſs; ſo you will ſooner or later meet again with the pungent liquor, which contains the third ſalt, as you did in the former ſhootings from the *bittern*, from which the pure *Sal-catharticum* is as neceſſarily required to be freed, as from the common ſalt; a proof of which cannot be better determined, than by one of the experiments hereafter taken notice of, *viz.* that with the oil of vitriol, which will certainly ferment with this ſalt, if the ſea ſalt have not been well ſeparated therefrom, or if it ſtill contain ſome of the third ſalt. And when any of the cryſtalizations will not ſtand the teſt of this experiment, they ought to be diſſolv'd and ſhot again, as before; by which means the pure ſalt is to be obtain'd. This Mr. *Brown* does not mention, as a

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trial made uſe of at the ſalt works; but what he himſelf had by experience found to be true; and the ſame experiment will ſerve to diſtinguiſh a *Sal-mirabile*, made at theſe works, from that made with the oil of vitriol and common ſalt: The account they give of it is, as follows; they take any quantity of coarſer grain'd cryſtals, boil'd from the *bittern*, which, when diſſolv'd and evaporated, more than they would otherways do for making the *Sal-cartharticum*, they throw into a wooden bowl, with ſome oil of vitriol, where it ſtands for ten days, and ſhoots into large tranſparent cryſtals, like the *Sal-mirabile*: But as this ſalt, by this method, is not ſufficiently ſaturated with the oil of vitriol (if they uſe any) ſo it is eaſily diſcovered by the oil of vitriol, which will readily ferment with it; whereas, it has no effect on the other *Sal-mirabile*, made as above.

Mr. *Brown* having receiv'd from *Newcaſtle* the ſeveral ſhootings of ſalts from their *bittern*, as alſo ſome of the *bittern* itſelf; from each of them he obtain'd a pure *Sal-catharticum*, as alſo the like kind of third ſalt, as that from the *Lemington* bittern. The method he took in doing it is agreeable to that already mentioned, and ſeveral years before tried at the ſalt works near *Portſmouth*. Mr. *Brown* was inform'd by Mr. *Cay*, that they ſometimes boil'd the *bittern*, without letting it ſtand any time to ſhoot of itſelf: The difference is not very material.

If this account be intelligible, the *Sal-catharticum* will no longer be a myſtery: And the next thing worth enquiring into is, whether this ſalt deſerve the reflections, that have diſcourag'd the preſcribing it? And why it may not paſs for a ſalt, as excellent in its kind, and be of the ſame nature and have the ſame properties, as that produced from the *Epſom*, or any other bitter purging ſprings.

And in order to prove it to be ſo, Mr. *Brown* gives a very ſhort abſtract of what Dr. *Grew* ſays of his ſalt, and then obſerves, how nearly the two accounts agree.

The Dr. in his treatiſe *de Natura Salis cathartici amari*, chap. 2. affirms, that in the evaporation of any of the bitter purging waters, they yield a *cremor*, as alſo a ſediment, both together weighing 6,8 or about 10 drachms, from a gallon of water; and that the leſſer part of this ſediment is in ſubſtance the ſame with this *cremor*; the reſt is all ſalt, but conſiſts of two ſorts, one a muriatic ſalt, the other proper or peculiar to thoſe waters.

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In the *Epsom* water, the muriatic ſalt is about a 20th part of the ſaline mixture; in the *Dulwich* it is in a greater proportion, and the ſame in ſeveral others; it is, both in its acrimonious taſte and figure of its cryſtals, not unlike common ſalt; the other ſalt is what the Dr. ſays is peculiar to the purging waters, and is made by evaporation and cryſtalization. In this preparation, firſt the earthy or plaiſtery part is to be ſeparated, next the muriatic ſalt, and laſtly, a brown and dark liquor from the proper ſalt of the waters.

In chap. 4 of the ſame part, having ſhewn the difference of the figures between the cryſtals of this ſalt and thoſe of alum, the Dr. goes on; neither is there any better ground to account the purging ſalt a ſpecies of common ſalt, from which being entirely freed, it differs as much in taſte, as from alum. And in the ſame chapter he ſays it will appear, the bitter purging ſalt, tho' it hath ſome qualities in common with other ſalts; yet is truly or ſpecifically different from them all: Thus far Dr. *Grew*.

Now, Mr. *Brown* does not ſee any thing in this account, but what will, *conſideratis conſiderandis*, very well agree with the purging ſalt from the ſea-water.

For, firſt, there is an earthy or plaiſtery part, contain'd in theſe waters, and this muſt be ſeparated: The very ſame is in the ſea water, and is precipitated in the boiling them down, as has been obſerv'd, and is call'd *ſcratch* by the workmen.

Next there is a muriatic ſalt, allow'd to be in theſe waters; in ſome more, in ſome leſs, and this is likewiſe to be ſeparated: The very ſame is done from the ſea water, tho' in a vaſtly greater proportion.

And laſtly, there is a black and dark liquor to be ſeparated; tho' this be but a dark way which the Dr. makes uſe of to expreſs himſelf, it cannot be better explain'd, than by what has been found to be fact in boiling down the waters at *Shooters-hill*; *viz.* that after ſeveral ſhootings of ſalt, by repeating the boiling of the waters, there would, at laſt, remain a liquor of a deep brown colour, which would no longer yield a cryſtaliz'd ſalt; but if boil'd up dry, would afford a ſalt of the ſame kind with the third ſalt already mentioned: And this, explaining Dr. *Grew*'s black and dark liquor, helps at the ſame time to prove, in this article too, that the ſea water yields the ſame kind of third ſalt.

Mr. *Brown* tried ſeveral of the Dr's experiments, by which he diſtinguiſhes his ſalt from other ſalts; ſuch as its not affecting the colour of ſyrup of violets; curdling of milk when boil'd; in the figure of its cryſtals; in its eaſy ſolution in the ſame quantity of water; in its coagulating with the oil of tartar *per deliquium*; in its calcination, and in the bitterneſs of its taſte as well before as after calcination, &c. and found this ſalt, thus ſeparated from the ſea water, anſwer all the trials. Mr. *Brown* here ſubjoins ſome few experiments, the Dr. had not taken notice of, and then leaves the whole to others to determine, whether there be any ſpecifical difference between theſe two ſalts?

In order to have a ſtandard for theſe experiments, Mr. *Brown* got Mr. *Hyet*, apothecary at *Epſom* (whoſe fidelity he could depend on) to boil down ſome of their waters, which he did from the well in the town, and ſent him a ſufficient quantity of the ſalts, to anſwer the purpoſe he wanted them for.

Mr. *Brown* likewiſe procur'd ſome of the firſt ſalts from the *Lemington bittern*: Theſe do not contain ſo much of what he has already diſtinguiſh'd by the name of the third ſalt, as he found the *Newcaſtle* ſalts do. This *Lemington* ſalt he, for diſtinction ſake, calls the firſt *Lemington* ſalt.

Part of this he diſſolv'd, and ſhot into pure *Sal-catharticum*; being freed both from the ſea ſalt and the third ſalt; and this he calls the ſecond *Lemington* ſalt.

He likewiſe procur'd from *Newcaſtle* the firſt ſalts, ſhot from their *bittern*, which he calls the firſt *Newcaſtle* ſalt.

Part of theſe he likewiſe diſſolv'd and ſhot, and obtain'd a pure *Sal-catharticum*, and this is what he calls the ſecond *Newcaſtle* ſalt.

He was oblig'd to make uſe of the *Sal-mirabile*, made from the oil of vitriol and common ſalt, that having been taken for the *Sal-catharticum*.

As alſo common ſalt, that having been repreſented, as the principal ſubſtance of the *Sal-catharticum*.

He took half an ounce of each of theſe ſalts, and diſſolv'd them in about two ounces of water to each half ounce of ſalt.

A ſmall quantity of each ſolution he poured into as many glaſſes, and drop'd into them all ſome butter of antimony: The precipitation, that followed, ſeem'd to be alike in them all; and upon dropping a little oil of vitriol into each, what was

was precipitated being more powerfully attracted by the oil, the ſeveral liquors became clear: Theſe are the two only experiments, in which he found the conſequences ſo much alike in them all.

In the following experiments the *Sal-mirabile* is ſufficiently diſtinguiſh'd from all the reſt.

Slices of galls put into theſe ſeveral ſolutions had no manner of effect upon any, excepting that of the *Sal-mirabile*, which is ſoon tinged of the colour of ſack, or rather deeper.

Sp. Sal. Arm. c. Tart. dropped into the ſeveral ſolutions, turns them all milky, excepting that of the *Sal-mirabile*, which keeps its tranſparency.

The *Sp. Salis armoniac c. cale*, the oil of tartar *per deliquium*, the tincture of cochinel in ſpirits of wine, do, each of them, us'd after the ſame manner, ſufficiently diſtinguiſh the *Sal-mirabile* from all the reſt.

In the following experiments, the *Epſom* ſalt, the ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt, agree together and differ from the common ſalt, the firſt *Lemington* ſalt and firſt *Newcaſtle* ſalt.

In the ſeveral ſolutions he dropp'd a ſolution of ſilver in *Aqua-fortis*, from which follow'd theſe conſequences; *viz.* the ſolution of the *Epſom* ſalt, ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt, became equally milky, before the precipitation. The ſolution of the ſea-ſalt and firſt *Newcaſtle* ſalt, let the precipitation paſs, without receiving any milky tincture: The firſt *Lemington* ſalt, as containing leſs of the third ſalt, than the firſt *Newcaſtle* ſalt did, took a little milky tincture. The precipitation fell nimbly thro' the ſolution of the *Sal-mirabile*, leaving it milky.

In the condition theſe were in, he pour'd ſome oil of tartar *per deliquium* to each of them; upon which, after ſome time, a blueiſh ſcum aroſe on the ſurfaces of the *Epſom* ſalt, ſecond *Lemington* ſalt and ſecond *Newcaſtle* ſalt: There likewiſe appear'd a little on the firſt *Lemington* ſalt; but not any on the reſt.

A ſolution of corroſive ſublimate was made in water, 10 drops of which, mix'd with the ſeveral ſolutions, produced little or no alteration; but upon dropping in the oil of tartar *per deliquium*, the following appearances were produced: In the ſolution of the *Epſom* ſalt, ſecond *Lemington* ſalt

salt and second *Newcastle* salt, the precipitations were red: In the solution of the common salt, and first *Newcastle* salt, the precipitations were white: In the solution of the first *Lemington* salt, the particles precipitated, approach'd pretty near the colour of the three first.

He took some of these several salts in substance, and upon each of them he pour'd a little oil of vitriol, which is one of the experiments Dr. *Grew* tried upon his salt, and which he says causes a moderate ebullition, whereby it appears to partake of an alkaline principle. But without looking for this alkaline principle from its fermenting with an acid (terms justly exploded by the learned Dr. *Friend* in his *Prælectiones Chem.*) Mr. *Brown* is apt to think, that the salt he tried the experiment on, had not, according to his own directions, been thoroughly separated from his muriatic salt: For, this oil pour'd on the *Epsom* salt, second *Lemington* salt and second *Newcastle* salt, produced no sensible fermentation: It acts with violence on the sea salt, forcing off its acid spirit with an intolerable *gâs*. It had the same effect in proportion on the first *Lemington* salt and first *Newcastle* salt; none at all on the *Sal mirabile*, as being a sea salt already saturated with the oil.

What Mr. *Brown* has all along call'd the third salt, answers in most of these experiments to the sea salt, and yet has some properties exceedingly different therefrom. To those he has already mentioned may be added the following ones; it will not decrepitate like sea salt; it readily melts, when put in a crucible in the fire; and when calcin'd till red-hot, affords a calx equal to, if not stronger than a lime-stone, and ferments violently, as well with water as with oil of vitriol. This calx, when expos'd to a moist air, will part of it run *per deliquium*, but not so soon as before calcination. All these properties differ in every respect from the common salt; which left Mr. *Brown* still in doubt what to call it; as also how far experiments of this kind may be deemed conclusive.

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An Account of Observations made with Mr. Hadley's *reflecting Telescope; by Mr.* James Pound. Phil. Transf. N° 378. p. 382.

IT were to be wish'd, that together with the particular description, given in Phil. Transf. N° 376, of the curious mechanism of that catadioptric telescope, made by Mr. *Hadley*, and by him presented to the Royal Society, a full account of what observations he had made therewith, had been communicated, whereby the public might at length have been appris'd of the usefulness of an invention (worthy of its great author Sir *Isaac Newton*) which, perhaps, from the vain attempts, made by some, of putting it in practice, hath lain neglected these 50 years: For, it is so long since it was first publish'd in Phil. Transf. N° 81.

Mr. *Hadley* has sufficiently convinced us, that this noble invention does not consist in bare theory; and it is to be hop'd, that he, or some other such curious persons, will in a short time find out a method, either of preserving the concave metal from tarnishing, or of cleaning it easily, when tarnish'd, or else of making a good concave *speculum* of glass, quick-silver'd on the back part. When a method for either of these shall be discovered, it is not doubted but that the old dioptric telescope will for the most part be laid aside, and this catoptric one be chiefly in use among practical astronomers; inasmuch as several inconveniencies and difficulties, unavoidable in the management of the former, especially when long, are in this latter entirely avoided.

It is no small conveniency, that by means of these reflecting telescopes, whose length exceeds not five feet (and which may be managed at a window within the house) celestial objects appear as much magnified and as distinct, as they do thro' the common telescope of more than 100 foot in length.

Mr. *Bradley*, *Savilian* Professor of Astronomy, and Mr. *Pound*, having compar'd Mr. *Hadley*'s telescope (in which the focal length of the object metal is not quite five feet and $\frac{1}{4}$) with the *Huygenian* telescope, the focal length of whose object-glass is 123 feet, found, that the former would bear such a charge, as to make it magnify the object as many times as the latter with its due charge; and that it represents objects as distinct, tho' not altogether so clear and bright; which may be occasioned partly from the difference of their apertures (that of the *Huygenian* telescope being somewhat the larger) and

and partly from several little spots in the concave surface of the object metal, which did not admit of a good polish.

Notwithstanding this difference in the brightness of the objects, they could see with this reflecting telescope whatever they had hitherto discovered by the *Huygenian*; particularly the transits of *Jupiter*'s satellites and their shadows over that planet's disk; the black list in *Saturn*'s ring and the edge of *Saturn*'s shadow, cast on his ring, as represented Fig. 4 Plate VIII.

They also observed with it several times the five satellites of *Saturn*; in viewing of which this telescope had the advantage of the *Huygenian*, at that time when they compared them: For it being in summer, and the *Huygenian* telescope being managed without a tube, the twilight prevented them from seeing in this some of those small objects, which at the same time they could discern with the reflecting telescope.

Observations on the Satellites *of* Jupiter *and* Saturn, *with the same Telescope; by Mr.* John Hadley. Phil. Trans. N° 378 p. 385.

MR. *Hadley* gave the *Royal Society* an account of some of the most remarkable observations, he had made with his reflecting telescope, before he presented it to them.

In observing *Jupiter*'s satellites he had seen distinctly the shadows of the first and third, cast upon the body of the planet Mr. *Folkes* and Dr. *Jurin*, being present, affirm'd, that Mr. *Hadley* had likewise shewn them the shadow of the third satellite thro' the same telescope.

In observing *Saturn* in spring 1723, at a time when that planet was about 15 days past the opposition, he saw the shadow of the planet, cast upon the ring, and plainly discerned the ring distinguished into two parts, by a dark line, concentric to the circumference of the ring: The outer or upper part of the ring seemed to be narrower than the lower or inner part, next the body of the planet, and the dark line, which separated them was stronger next the body, and fainter on the outer part towards the upper edge of the ring. Within the ring he discerned two belts, one of which cross'd *Saturn*, close to its inner edge, and seemed like the shadow of the ring upon the body of *Saturn*; but when he considered the situation of the sun, in respect to the ring and *Saturn*, he found that belt could not arise from such a cause.

He affirmed that at times he had observed with this telescope three different satellites of *Saturn*; but could never have the good fortune to see all the five.

August 1723 Mr. *Hadley* adds, that he several times observ'd the shadow of the first, second and third satellites of *Jupiter* pass over the body of that planet; and that he had seen the first and second appear, like a bright spot upon the body of *Jupiter*, and that he could keep sight of them there for about a quarter of an hour, from the time of their entering on his limb.

Jupiter's satellites had of late years been so situated, with regard to the earth and *Jupiter*, that Mr. *Hadley* had not had sufficient opportunity of observing the transit of the fourth satellite or of its shadow.

The dark line on the ring of *Saturn*, parallell to its circumference, is chiefly visible on the *ansæ*, or extremities of the elliptic figure, in which the ring appears; but several times he could trace it very near, if not quite round; particularly in *May* 1722, he could discern it without the northern limb of *Saturn*, in that part of the ring that appeared beyond the globe of the planet. The globe of *Saturn* (at least towards its limb) reflects less light than the inner part of the ring, and he had sometimes distinguished it from the ring by the difference of colour.

The dusky line, which in 1720 he observed to accompany the inner edge of the ring cross the disk, continued close to the same, tho' the breadth of the ellipsis was considerably increased since that time.

An Account of an extra-uterine Fœtus, *that had continued five Years and ½ in the Body; by Dr.* Houstoun. Phil. Trans. N° 378. p. 387.

IN *August* 1717 Dr. *Houstoun* was sent for to a woman near *Newport market* in *London*, who had been married 18 years to a native of the *East-Indies*, by whom she had eight children, besides two miscarriages: At this time she was with child by a second husband, a vigorous young man.

She was near her full time and felt her pains for several days, which returning by intervals, she concluded, would, as usual, bring on her delivery. Her mother and the midwife, apprehending no difficulty, assured those about her, that only time was wanting.

But the Dr. found upon examination, that her *uterus* was no bulk to contain a child near its time; and that its neck, an uncommon hardneſs, was alſo cloſed up ſo ſtraitly, as give not the leaſt admiſſion, even to a ſmall probe or knittin needle.

Upon this the Dr. affirm'd, that her delivery was impoſſibl becauſe the child was not within the *uterus*, but between it an the guts; that it might be extracted by a paſſage to be made f it, without any great pain and with ſafety to the mother. T Dr. offered to undertake it, and aſſured them this was the onl opportunity of giving her any relief.

However the Dr's aſſiſtance was rejected. At that time, pro bably, it would have been ſucceſsful; for, ſhe was a ſlende well ſhaped woman, in good habit of body and of a ſprightl diſpoſition.

A year after, the Dr. was deſired to viſit her again: Wh he found her much diſordered with a growing impoſthumatio in her belly: He ordered her ſome cordial ſtomachics, *caſſ* and ſuch gentle lenitives; which met with ſucceſs beyond e pectation: So that, by means of a regular diet and the watch ful care of a very tender mother, ſhe was reſtored to ſuc ſtrength, that ſhe went chearfully abroad and re-applied herſe to buſineſs.

About 15 months from the time the Dr. viſited her firſt, h mother came to him to intreat his aſſiſtance; ſhe complained great pain in the lower part of the *abdomen*, and he found tumour of a conical figure, projecting about an inch beneath th navel: Its inflammation, with tenſion and feveriſhneſs attendin it, ſo plainly indicated ſuppuratives, that he was not ſurpriſe to hear, in a few days, that it had broke, as he had wiſh'd.

The Dr. propoſed to lay it open, both to give a free exi and prevent its becoming fiſtulous; but the patient was appr henſive, that he would, as ſhe call'd it, cut open her belly: S that not being able to prevail with her, he ordered her a pot unguent and ſome plaiſters.

The ulcer ſoon grew fiſtulous, and ſo continued till ſhe die which was on the 23. of *March* 1723. in the 41ſt year of h age.

For above five months before her death, ſhe voided her ex crements by this vent, and all the ſoft parts of the *fœtus*, wit ſome ſmall bones of its fingers: But the reſt of the ſkeleton r maining entire, he extracted out of her body, together with th

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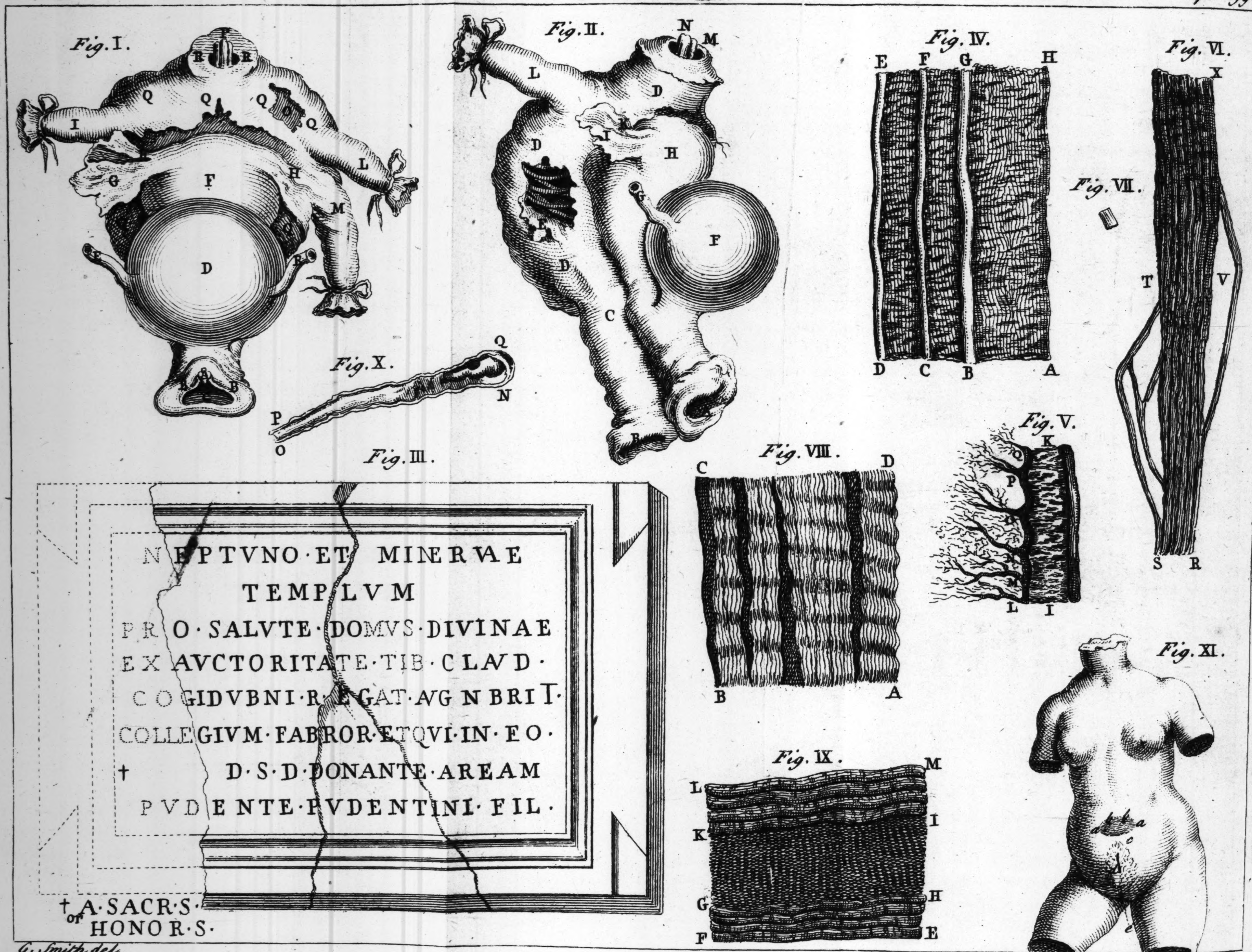


G. Smith del. Hulett Sculp.

vagina, *uterus*, *rectum*, &c. wherein it had involved itself, as may be seen more particularly in the Fig. annexed.

A Fig. 1. Plate IX. represents the *clitoris*; B B the *nymphæ*; C the *rima* or entery into the *vagina*; D the bladder; E E the ureters cut off; F the *uterus* sound and entire; G the left *ovarium*; H H the *tubæ*; I part of the *colon* cut off; K part of the *peritonæum*; L part of the *ileum* cut off; M part of the *cæcum*; O part of the *cranium* that plainly appears; P the ulcer, thro' which the *fæces* passed, with some small bones; Q Q Q Q the bulky mass, wherein the skeleton is contained between the *uterus*, part of the *vagina* and *rectum*; R R part of the *cutis*.

She was full nine months gone in *August* 1717, and she died *March* 23, 1723, on which day the Dr. took it out.

Fig. 2. represents a lateral view; A the orifice of the *vagina*; B the *anus*; C the *rectum*; D D D the mass wherein the skeleton is contained; E the ribs of the *fœtus* plainly appearing; F the bladder laid aside; G the *ureter* cut off; H the *uterus*; I the *ovarium*; K the *tuba*; L part of the *colon*; M the ulcer, thro' which the excrements passed, about one inch below the navel; N bones in the orifice of the ulcer.

An Account of a Roman Inscription, *found at* Chichester; *by Mr*. Gale. Phil. Trans. N° 379. p. 391.

THE inscription, represented Fig. 3. Plate IX. as curious as any hitherto discovered in *Britain*, was found at *Chichester*, in digging a cellar under the corner house of St. *Martin's-lane*, on the north side as it comes into *North-street*: It lay about four foot under ground with the face upwards, whereby it received a great deal of damage from the picks of the labourers, as they endeavoured to raise it; for, besides the defacing of several letters, what was here disinterr'd of the stone was broke into four pieces; the other part of it, still wanting, is, probably, buried under the next house: The inscription is cut upon a grey *Sussex* marble, the length of which was six *Roman* foot, as may be conjectured by measuring it from the middle of the word *templum* to that end of it which is entire, and is not quite three foot *English* from the said point; its breadth is two foot $\frac{3}{4}$ of the same measure; the letters are beautifully and exactly drawn, those in the two first lines three inches long, and the rest two inches and $\frac{1}{4}$.

Mr. *Gale* and Dr. *Stukely* took an accurate view of this marble, and in order to be as sure of the true reading as possible,

wherever the letters were effaced, they impressed a paper with a wet ſponge into them, and by that means found thoſe in the fifth line to be, as expreſſed in the Fig. and not as in other copies, that have been handed about of this inſcription.

The only letter wanting in the firſt line is an N, before EPTVNO; and ſo there is no difficulty in reading that: As to the ſecond line, tho' it be more uſual in inſcriptions of this nature to expreſs the donation by the word SACRUM only, referring to the temple or altar dedicated; yet we have ſo many inſtances, in *Gruter's corpus Inſcriptionum*, of TEMPLUM and ARAM, alſo cut on the ſtones, that there is not the leaſt occaſion to ſay any thing farther upon that point.

The third line can be no otherways filled up, than Mr. *Gale* has done it by the pricked letters: He owns, however, that he had ſome ſcruple about the phraſe DOMUS DIVINA, the ſame thing as DOMUS AVGVSTA, the imperial family, which he cannot ſay occurs, with any certainty, as to the time it was uſed in, before the reign of *Antoninus Pius*, from whom down to *Conſtantine the Great*, it is very frequently met with in inſcriptions. This kept him ſome time in ſuſpenſe, whether this found at *Chicheſter* could be of ſo early a date, as the time of *Claudius:* But as we find ſeveral inſcriptions in *Gruter* with thoſe words in them, or J. H. D. D. *In honorem domûs divinæ*, which is much the ſame thing, without any mark of the time, when they were cut, they may have been before the reign of *Antoninus Pius*, and then only came into more general uſe; and as the time that *Cogidunus* lived in, will not ſuffer this to be of a later ſtanding, Mr. *Gale* thinks it may be offered as an authority for the uſe of this piece of flattery to the emperors, long before that excellent prince came to the purple.

The third line as Mr. *Gale* takes it, was EX AUCTO. RITATE. TIB. CLAUD. and the fourth COGI. DUBNI R. LEG. &c. that is, *ex authoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Britanniâ*, for the following reaſons; we are informed by *Tacitus* in *Vitâ Agricolæ* cap. 14. that after *Britain* had been reduced to a *Roman* province by the ſucceſsful arms of *Aulus Plautius* and *Oſtorius Scapula*, under the emperor *Claudius*; *Quædam civitates Cogiduno Regi erant donatæ; is ad noſtram uſque Memoriam fidiſſimus remanſit, vetere ac jam pridem receptâ Populi Romani conſuetudine, ut haberet inſtrumenta ſervitutis & Reges:* This *Cogidunus* ſeems to be the ſame perſon with *Cogidubnus*

dubnus in the inscription, the letter B in the third syllable making little or no difference in the word; especially if pronounced soft, as it ought to be, like an V consonant.

It is so well known to have been the custom of the *Roman Liberti* and *Clientes* to take the names of their patrons and benefactors, that it would be needless to prove the constant usage of that practice: Now, as this *Cogidubnus*, who, in all probability, was a petty prince of that part of the *Dobuni*, that submitted to *Claudius*, and one that continued many years faithful to him and the *Romans*, vide *Tacitus ut supra*, had the government of some part of the island given him by that emperor, nothing could be more grateful in regard to *Claudius*, nor more honourable to himself, after he was *romanis'd*, than to assume the names of a benefactor, to whom he was indebted for his kingdom; and so called himself TIBERIUS CLAUDIUS COGIDUBNUS.

Mr. *Gale* supposes him to have been a *Regulus* of the *Dobuni*; because we are told by *Dion Cassius lib.* 60, that *Aulus Plautius* having put to flight *Cataratacus* and *Togodumnus*, sons of *Cunobelin*; part of the *Boduni* (the same people as the *Dobuni*) who were subject to the *Catuellani*, submitted to the *Romans*; and the name *Cogidubnus* or *Cogiduvnus* Coc o Dubn or Dubn, vide *Baxteri Glossar. in verbis* COGIDUMNUS and DOBUNI, signifying expresly in the *British* language *Princeps Dobunorum*, seems to put the matter out of all doubt.

How far his territories extended, it is impossible to determine: Bishop *Stillingfleet Orig. Britan. p.* 63. supposes them to have lain in *Surrey* and *Sussex*; *Sussex* was certainly part of them; since the temple, mentioned in this inscription, was erected in it by his authority; and it is not unlikely, that besides the *Regni*, who were the people of those two counties, he might have assigned to him that part of the *Dobuni* that had submitted to the *Romans*, which seems to have been his own principality, together with the *Ancalites*, *Bibroci* and *Segontiaci*, whose countries lay between the *Dobuni* and *Regni*; the words *Civitates quædam* in *Tacitus*, not importing some few towns, but several people; the word *civitas* always signifying a people in that historian.

Before Mr. *Gale* proceeds any farther, he observes, that *Togodumnus* and *Cogidumnus* (tho' their names be so much alike) were two distinct persons; the first, son of *Cunobelin*, king of the *Trinobantes*, vanquished and kill'd in battle by *Aulus*

Plautius;

Plautius; the second a prince that submitted to *Ostorius Scapula*, and continued in his fidelity to the *Romans in nostram usque memoriam*, says *Tacitus*, and who was born at the latter end of *Claudius*'s reign: So that *Togodumnus* was, probably, dead, before *Cogidumnus* had his government conferred upon him.

Mr. *Gale* calls it his government: For, tho' by the letter .R. in the inscription, with a point both before and after it (whereby it plainly denotes an entire word of itself) it may seem that it was intended for COGIDUBNI REGIS; and he believes was so with respect to his former dignity; yet it is evident, that he had condescended to take the title of LEGATUS AUGUSTI IN BRITANNIâ from *Claudius*; and that too must have been only over those people, the government of whom was given him: *Aulus Plautius*, *Ostorius*, *Scapula*, *Didius Gallus*, *Avitus Veranius* and *Suetonius Paulinus*, having about this time the supreme command, successively in this island; the second and last of which are expresly called *Legati* by *Tacitus*, *lib.* 12. *Ann. cap.* 32 and *Vit. Agricol. cap.* 15. The *Legati Cæsaris* or *Augusti* were those, *qui Cæsaribus subditas regebant Provincias.*

The sixth line has lost at the beginning the letters COLLE; but so much remains of the word as makes it, undoubtedly, to have been, when entire, COLLEGIUM, and the following letters are an abbreviation of FABRORUM.

These colleges of artificers were very ancient at *Rome*, even as ancient, as their second king *Numa Pompilius*, if we may believe *Plutarch* in *Vit. Numæ*, who tells us, that the people were divided by him into what we at this day call *Companies of tradesmen*, and he mentions the Τέκτονες or *Fabri* among them; tho' *Florus* lib. 1. cap. 6. says, that *Populus* Romanus *a Servio Tullio relatus fuit in censum, digestus in classes, curiis atque Collegiis distributus.* But as the power of the *Romans* extended itself, it carried the arts of that great people along with it, and improved the nations it subdued, by civilizing and teaching them the use of whatever was necessary or advantagious among their conquerors; from which most wise and generous disposition, among other beneficial institutions, we find these *Colleges* to have been established in every part of the empire, from the frequent mention of them in the inscriptions, collected by *Gruter*, *Spon* and other antiquaries.

Several sorts of workmen were included under the name *Fabri*, particularly all those concerned in any kind of buil-

ing; whence we meet with the *Fabri Ferrarii*, *Lignarii*, *Tignarii*, *Materiarii*, *Navales* and others: The *Navales* may have been the authors of dedicating this temple to *Neptune*, having ſo near a relation to the ſea, from which the city of *Chicheſter* is at ſo ſmall a diſtance, that, perhaps, that arm of it, which ſtill comes up within two miles of its walls, might formerly have waſhed them: The reſt of the fraternity might very well pay the ſame devotion to *Minerva*, the Goddeſs of all arts and ſciences, and patroneſs of the *Dædalian* profeſſion.

As no leſs than five letters are wanting at the beginning of the ſixth line; there cannot be fewer loſt at the beginning of the ſeventh, where the ſtone is more broke away than above; ſo that, probably, there were ſix when it was entire. What is left of them is only the top of an S: Mr. *Gale* does not, therefore, take upon him to affirm any thing, as to the reading of them, being ſo entirely defaced: Perhaps, it was, A SACR. S. *i. e.* A *ſacris ſunt*; or perhaps HONOR. S. i. e. *Honorati ſunt*: As to the former, we find theſe *Collegia* had their *Sacerdotes*; therefore, *qui a ſacris ſunt*, which is found in inſcriptions (vide *Gruter Corp.* 29.8—121.1—632.1.) would be no improper term to expreſs them; or it might have been SACER. S. i. e. *Sacerdotes ſunt*, ſince we find ſuch mentioned in the following inſcriptions in *Spon's Miſcell. Erudit. Antiquit.* p. 58.

MAVORTI SACRUM
HOC SIGNUM
RESTIT. . . . .
COLL. FABR. ARI
CINORUM ANTIQUISS.
VETUSTATE
DILAPSUM ET
REFECER. CUR. L. LUCILIUS
LATINUS PROC. R. P. ARIC
ET T. SEXTIUS MAGGIUS
SACER. COLL. EJUSD.

i. e. *Mavorti ſacrum hoc ſignum reſtituit Collegium* Fabrorum Aricinorum *antiquiſſimum, vetuſtate dilapſum,* & *refecerunt. Curabant Lucius Lucilius Latinus, Procurator Reipublicæ Aricinorum,* & *Titus Sextius Maggius* Sacerdos *Collegii ejuſdem.*

Ibid.

Ibid. p. 64. in the following inſcription

L. TERTENI AMANTI
SACER. COLL. LOTORUM
II VIR. C. SARTIUS C. F.
ITERINUS ET L. ALLIUS
PETELINUS D. D.

i. e. *Lucius Tertenius Amantius* Sacerdos *Collegii Lotorum, Duumviri Caius Sartius, Caii filius, Iterinus, & Lucius Allius Petelinus dedicaverunt.*

As to the latter, thoſe members of the college, that had paſſed thro' its chief offices, as that of *Præfectus* or *Magiſter quinquennalis*, had the title of HONORATI conferred upon them: Several of theſe HONORATI are mentioned in *Gruter*; particularly a long catalogue of them *in Collegio Fabrorum Tignariorum*, p. 268.1. and in *Reineſius's ſyntagma* p. 605. there is the following inſcription.

EPAGATHO TURANNO
HONORATO COLLEGI
FABRUM TIGNARIORUM
ROMANENSIUM, &c.

So that the vacuity in our inſcription might very well be filled up with one or other of theſe words, and the three next lines that follow them D. S. D. i. e. *de ſuo dedicaverunt*, will agree with either of them and with what preceeds them.

The laſt line has been PUDENTE PUDENTINI FIL*io*; but there muſt have been a letter or two of the *prænomen* at the beginning of it; unleſs it were ſhorter than the reſt at that, as well as at the latter end of it; and from what Mr. *Gale* has ſaid, the whole may be read, as follows.

Neptuno & Minervæ Templum pro ſalute domus divinæ, ex authoritate Tiberii Claudii Cogidubni Regis, Legati Auguſti in Britanniâ, Collegium Fabrorum, & qui in eo a ſacris (or *honorati*) *ſunt de ſuo dedicaverunt, donante aream Pudente Pudentini filio.*

Chicheſter, by this inſcription found at it, muſt have been a town of note, very ſoon after the *Romans* had ſettled here, and in proceſs of time it ſeems to have been much frequented, by the *Roman* roads, ſtill viſible, that terminate here from *Portſmouth*,

mouth, *Midhurst* and *Arundel*; tho', what is very strange, we have no *Roman* name now known for it. Mr. *Gale* once thought it might have put in its claim for *Anderida*, which our Antiquaries have not yet agreed to fix any where, being situated very near, both to the *Sylva Anderida* and the southern coast of the island, the two properties of that city, vide *Camd. Brit.* and *Somner's Roman Ports and Forts*: But *Henry* of *Huntingdon*, who liv'd in the time of *Henry* II. telling us, p. 312. that the *Saxons* so destroy'd *Andredecester*, that *nunquam postea reædificata fuit, & locus tantum quasi nobilissimæ urbis transeuntibus ostenditur desolatus*, vide *Phil. Trans.* N° 356, it could not be *Chichester*: For, that was not only rebuilt before his time, but a place of such note, that when the Bishops soon after the conquest A. D. 1076, remov'd their churches from small decay'd towns, where several of them were then seated, *in urbes celebriores*; *Stigand*, then Bishop of *Selsey*, settled his Episcopal chair at that place.

Mr. *Gale* concludes with observing, that when this inscription was dug up, there were also two walls of stone, discover'd close by it, each three foot thick; one running north, the other east, and joining in an angle, as *North-street* and St. *Martins-lane* now turn, which, in all probability, were part of the foundations of the temple, mentioned on the marble.

Of the Structure of the Diaphragm; *by M.* Leewenhoeck. Phil. Trans. N° 379. p. 400. *Translated from the* Latin.

IN *Phil. Trans.* N° 377. M. *Leewenhoeck* gave his opinion about a disorder he was sies'd with, which he suppos'd had been owing to the bad disposition of his diaphragm.

In order to remove all doubts about this matter, he procured the diaphragm of a sheep a year old, part of which he cut into small pieces, and viewing them very carefully with his microscope, he found, that the diaphragm consists partly of very fine fibrils, which appear to the naked eye to be about a hair's breadth distant from each other.

Upon placing different pieces of the diaphragm before different microscopes, and carefully viewing them, he positively concluded, that these fibrils arose from fleshy parts, interwoven with the diaphragm about the ribs; and suppos'd, that the said fibrils serv'd the diaphragm instead of tendons:

Theſe tendons, as M. *Leewenhoeck* calls them, are not equally near each other throughout the whole diaphragm; for ſome are a little thicker than others, tho' near each other.

Upon duly conſidering the laſt mentioned fibrils and the membrane extended between them, he found, that the greateſt part of the diaphragm conſiſted of theſe fibrils and the ſaid membrane; beſides a great number of blood-veſſels and a large quantity of fat contain'd therein.

M. *Leewenhoeck* was very much ſurpris'd at the vaſt number of little *rugæ* or folds in the membrane between the pretended tendons; theſe *rugæ* he ſuppoſed deſign'd by nature for contracting the diaphragm, when extended in inſpiration, and by that means contributing to expel the air convey'd into the lungs.

To ſhew the contexture of the diaphragm, he caus'd delineate a ſmall portion of the diaphragm of the ſaid ſheep, which is repreſented by A B C D E F G H, Fig. 4. Plate IX. where B G, C F and D E are three tendons, as they appear'd thro' the microſcope: Between theſe tendons may be ſeen the intermediate membrane, of which the firſt tendons, (as has been already obſerv'd) the greateſt part of the diaphragm conſiſts.

In order to form a judgment of the true bigneſs of Fig. 4. the real ſize of the piece delineated is repreſented by the ſpace between Y Z in Fig. 7.

Moreover, he obſerv'd, that the ſaid membrane was perforated with a vaſt many, and thoſe very ſmall holes, which opened from the *abdomen* into the breaſt, and from the breaſt again into the *abdomen*. Being hitherto of opinion, that it was impoſſible for any liquor to be convey'd from the breaſt into the *abdomen*, or from the *abdomen* into the breaſt, he now thought it better, to ſuſpend his judgment about that matter: Yet he reaſoned thus, there are, perhaps, inconceivably ſmall veſſels in our lungs; for, if we conſider, how great a quantity of moiſture is expir'd out of our lungs into the open air, in compariſon of the ſmall quantity inſpir'd into them, we will eaſily incline to that opinion.

M. *Leewenhoeck* ſeveral times obſerv'd, that ſome veſſels creep tranſverſely over thoſe fibrils, he calls tendons.

In another piece of the ſame diaphragm, that ſtood before his microſcope, he could plainly obſerve the ſtructure and origin of the little membranes, which, as has been ſaid, la[y]

lay between the fibrils or pretended tendons, and were furnish'd with a vast many folds, as has been likewise observ'd; all which are represented at LK Fig. 5. but the fibrils or small tendons at LMNOPQ; tho' these fibrils were larger here, than ever he had observ'd before.

Moreover, he tore that membrane asunder (of which the greatest part of the diaphragm consists) notwithstanding it was so thin; a small portion of which he caus'd to be delineated, as it appear'd thro' the microscope, and represented Fig. 6. This he did in order to shew (as much as possible) the torn tendons and membranes; a small portion of an extended tendon is represented by RSTV, where are shewn the very minute fibrils, of which the said portion of the tendon consists: In the same figure VT and WX represent a portion of the tendon at rest.

It is scarce possible to explain, or sufficiently comprehend, what an incredible number of fibrils, vessels and particles are here join'd together and contribute to form that very fine membrane, the diaphragm. M. *Leewenhoeck* supposes, that in strong inspiration, all these *rugæ* or corrugated *Sinus*'s in the diaphragm become flat and plain; and that in evaporation, the diaphragm is again corrugated, as was observ'd above.

While, therefore, we see so great a quantity of fat generated, and collected in the diaphragm; and moreover consider, that the substance of the diaphragm may on both sides become so thick with fat, as to swell out; we may easily conceive, how the expansion and contraction of the diaphragm may by that means be hinder'd and retarded; and consequently, a difficulty of breathing arise: And this is the reason that fat people are short winded. Yet that the diaphragm be lin'd with some fat is necessary, as it expands 90 times in an hour, and as often contracts; and these several motions are much assisted by the fat.

Tho' M. *Leewenhoeck* several times endeavoured to discover, how the fleshy parts of the diaphragm, which (as has been said) lie next the ribs, became a membrane, or how the membrane of the diaphragm arose from them; after several trials, he could not satisfy himself.

Now, he began to suspect, whether these tendons were not furnish'd with vessels; and tho' he could hardly promise so much to himself from his microscope; yet the third day after, he repeated his observations, and cut the membranes

transversely, in order to discover the blood-vessels in the membrane of the diaphragm, which, as has been said, was full of *rugæ*: At length he observ'd blood-vessels, extended along the tendons, nay, 8 or 10 close together: Afterwards he turn'd his eyes to view the membrane, that covers the tendons, and here he observ'd a vast number of very small vessels, many of which lay included in *areolæ*, as it were; and M. *Leewenhoeck* judged them to be the vessels of the tendons cut transversely. In order to have an eye-witness of all this, he gave the microscope (before which the said portion of the diaphragm stood) to his surgeon; who affirm'd, that M. *Leewenhoeck*'s description exactly agreed with what he saw.

M. *Leewenhoeck*, not satisfied with this observation, procur'd the diaphragm of an ox, from which cutting off about the breadth of a palm, at the fleshy parts, next the ribs, he found it about four times thicker than that of a sheep, and that this thickness is mostly owing to the fat.

Afterwards he cut off a thin slice, and as often as he cut off one somewhat thicker, he cut thro' fat, extending towards the middle of the diaphragm, where the diaphragm is pretty thick, and has some fat. The diaphragm on both sides, that is, in its upper and lower parts, consists of four distinct membranes and their tendons, which last lie inclos'd, as it were within the membranes.

M. *Leewenhoeck* was surpris'd at the vast number of these tendons; nor did it seem credible, that there should be such variety of parts in a body, as the diaphragm, whose structure and functions are so little known.

To shew the structure of the diaphragm of an ox, he caus'd part thereof to be delineated, as represented by ABCD Fig. 8. where are shewn first four parts of distinct tendons which lie close to each other in great numbers, and are likewise represented in the figure of the sheep's diaphragm. But such minute tendons (as he calls them) are spread throughout the whole substance of the diaphragm of an ox which, together with the membranes, must incessantly be alternately expanded and contracted in a well dispos'd body; all which is pretty accurately delineated in Fig. 8.

M. *Leewenhoeck*, by repeated observations, likewise found to his satisfaction, in the diaghragm of a sheep, those very fine fibrils, as also the small *rugæ* or folds of the membranes which he did not only himself observe with pleasure, but

shew'd

shew'd them to others. Moreover, cutting these tendons, and very thin particles transversely, in order to find, whether they were perforated and furnish'd with cavities, he observ'd such a vast number of very small vessels, as is scarce credible to any, who had not seen them, running across the diaphragm; and as far as he could judge, design'd by nature to convey the fat thro' all its parts, and supply the diaphragm with constant nourishment,

M. *Leewenhoeck* observ'd above, that the fat lay inclos'd in the diaphragm, and that four distinct membranes cover it on both sides; and these are so closely join'd together, that they seem'd to be but one single membrane.

M. *Leewenhoeck* placed before another microscope (that magnified something less than the former) a piece of the diaphragm of a mean thickness; in order to shew, how the membranes surrounded on both sides the fatty particles, or the fat of the diaphragm, so that it seems to be inclos'd within these membranes: This is represented by EFGHI KLM Fig. 9. where EFGH and IKLM shew the two membranes that consist of four parts each; GH and IK the place where the fat, so often mentioned, is inclos'd.

Afterwards M. *Leewenhoeck* endeavouring to cut into round pieces a very small portion of the diaphragm, it separated into two parts or *lamellæ*; whence he suspected, that the diaphragm was so form'd by nature, for the more easily distribution of the fat, thro' the substance of the diaphragm.

In Fig. 9. where the membranes are represented cut transversely, there appear some small corrugations, which he mostly observ'd in them. Afterwards he found that the little membranes, where the fat is collected, were fastened to very minute vessels, which by the exsiccation of the contiguous parts were broken asunder. M. *Leewenhoeck* takes the connection of these little membranes and vessels to be necessary on this account, *viz.* that the diaphragm, as often as it is extended, may not become quite flat, but almost expand into a globose cavity.

A great many people, may possibly think, that the diaphragm is a very thick membrane; whereas it is a very thin one; and to shew this, he caus'd delineate a little bit of the diaphragm of an ox, as it appears to the naked eye: It is true, the diaphragm is very strong, considering its thickness; which he takes to be owing to the great number of fine tendons.

NOPQ

NOPQ Fig. 10. represents a little bit of the diaphragm of an ox; where the fibril NO represents one side or half the thickness of the diaphragm; PQ the other half: The rest of the diaphragm, to wit, what lies between OP and NQ, is surrounded with fat or fatty particles; only that there lies some flesh between NQ, belonging to that next the ribs.

The oftener therefore, M. *Leewenhoeck* reflected upon the abovementioned disorder he was seised with, and which he attributed in a great measure to the diaphragm, he was persuaded physicians were mistaken, when they call the palpitation, we sometimes perceive in the region of the breast, a palpitation of the heart. M. *Leewenhoeck* takes such palpitations to arise from the bad disposition of the diaphragm; whether it be owing to a want of nourishment, or to an obstruction in some of these vessels, which are very numerous throughout the diaphragm: For, such an obstruction may easily cause convulsive motions in the abovementioned tendons; and M. *Leewenhoeck* takes this to have been the very cause of his disorder,

A preternatural Structure of the Parts of Generation *in a* Woman; *by Dr.* Huxham. Phil. Transf. N° 379. p. 408. *Translated from the* Latin.

ONE *A. B.* of the parish of *Lanteglass*, in the county of *Cornwall*, near *Fowye*, was married at 23 years of age; after which she conceived, and being conscious of the bad conformation of the genital parts, she had recourse to Mr. *Bonnett* of *Fowye*, a surgeon and man-midwife, who made the following observations.

In the place of the navel, in the middle of the *abdomen*, but a little lower, there was a prominent spongy mass, resembling a lump of flesh, lying transversely upon the *abdomen*, almost as big as a hen's egg and three fingers in length; with two small urinary passages from which the urine continually ouz'd, so that she could neither retain nor squirt it out; hence we may conclude, that the bladder (if there were any) wanted a *sphincter*. This spongy soft mass, being corroded with the acrimony of the urine, could hardly bear the gentlest touch; so that she was obliged to walk double, to avoid rubbing against it with her cloaths, and to wrap it up in soft linnen: Dr. *Huxham* takes this mass to be the navel-string, that was unskillfully cut and as unskillfully cured: For, there did not appear the least sign of a navel, any where else but here; and either the urine flow'd thro' the *urachus* which was perforated, and, probably divided

into

into two urinary paſſages, or at leaſt thro' two peculiar canals; however he rather inclines to the former opinion; becauſe we have accounts of the voiding of urine by the navel, and that in adults, vide *Hiſt. de l'Academie Roy. des Scien. Ann.* 1701.

A little way, below this maſs of fleſh was the orifice of the *vagina*; from which flowed the *menſes*; and by which ſhe was impregnated; tho' with little pleaſure in the time of coition, the extremity of the *glans* (much leſs the *penis* itſelf) being ſcarce able to enter this orifice: And the ſurgeon could hardly introduce his finger into it, in order to diſcover the neck of the *uterus*, which he could by no means feel; but he plainly felt, a thick membrane, that ſeparated this orifice from one lower, to be deſcribed anon

In the ſame place almoſt but ſomewhat higher, where in other women is the *foſſa magna*, was that other oblong orifice, ſcarcely admitting the tip of the little finger, pervious to the *rectum*, as was obſerved after the delivery, and this might, probably, be owing to the inciſion; no *ſphincter* was obſerved, but below that, the *rectum* terminated, as uſual, with its *ſphincter*.

This oblong orifice was almoſt two fingers diſtant from the orifice of the *vagina*, when the *abdomen* was moſt ſwelled; and between theſe intervened the abovementioned membrane internally, but the coalition, as it were, of the *labia* of this oblong fiſſure from the ſuperior part of the orifice externally.

There was neither *clitoris* nor *oſſa pubis*, unleſs, the ſhort *apophyſes*, jutting out from the lower part of each *os ilium*, might be called the rudiments of them. This was the caſe before delivery.

July 18. 1722, Mr. *Bonnett* was called at 11 o'clock at night, to aſſiſt in the delivery. Upon thoroughly conſidering every particular circumſtance, he found the *fœtus* fallen below the orifice of the *vagina*, and in vain he attempted to thruſt it up, by putting the woman in a proper poſture, by reaſon of the ſtrong motion of the *fœtus* and violent throws of the mother, who was now ſeiſed with convulſions, *ſyncope*, &c. The orifice of the *vagina* was ſcarce, if at all, dilated; ſo that every one expected ſhe would immediately expire.

In this melancholly ſituation, Mr. *Bonnett* was of opinion, that a doubtful remedy was to be preferred to none at all, ſince otherwiſe death was unavoidable; introducing his ſcalpel into the lower oblong orifice, he at once divided the coalition of the *labia*, and the intermediate membrane; ſo that the orifice of the *vagina* and the lower oblong orifice now became one: Upon

this,

this, he easily introduced his fingers, felt the internal orifice of the *uterus*, and dilated it a little, so that he could now feel the head of the *fœtus:* In short, putting his finger into its mouth, he at length extracted a live female child to the great surprise of the by-standers.

From the time of her delivery she was troubled with a *prolapsus uteri*; if not reduced for 8 or 10 hours and afterwards forcibly thrust up, from the abovementioned urinary passages two streams would squirt out to the distance of at least a foot; a manifest proof that there was some *cystis* for receiving the urine; otherwise the Dr. suspects they might be the orifices of the *ureters*, that terminated there.

Should any one ask, how this woman was impregnated? To this the Dr. answers, that the immission of the *penis* is not absolutely necessary in procreation, tho' the injection of the *semen* into the *vagina* be absolutely so; vide *Hist. de l'Acad. Royale des Sciences An.* 1712. as also *Mauriceau.*

A A Fig. 11. Plate IX. shews the spongy mass of flesh (which the Dr. takes to have been part of the navel-string) almost of the bigness of a hen's egg and three fingers in length; in it were two urinary passages *b b*, from whence the urine continually ouz'd, without being voided by any other orifice; *c* the orifice of the *vagina*, very much resembling the *anus* of a cock, and surrounded with a strong *septum*, as it were; by this the *menses* flowed and by it the woman was impregnated: This orifice, in the most violent throws, was scarcely at all dilated, tho' the *anus* was very much, from the violent contractions of the muscles of the *abdomen*, &c. *d* the place of the oblong orifice, scarce admitting the tip of the little finger; and into this (at the time of delivery) Mr. *Bonnett*, introducing his scalpel, cut it up as high as the superior orifice; so that now there was a prodigious large aperture here: The upper transverse orifice on the approach of the birth, was almost two fingers distant from the lower oblong orifice, Thro' this large aperture she often suffered a *prolapsus uteri*: From the time of the delivery, the *fœces* were partly voided by that part where the lower orifice was formerly; but this was not observed before the delivery, so that, probably, it was owing to the operation. This oblong orifice is represented in the Fig. somewhat higher than the life; *e* represents the *anus* a little more forwards than usual; by which the greatest part of the *fœces* was discharged: The *pudenda* was not thick of hair.

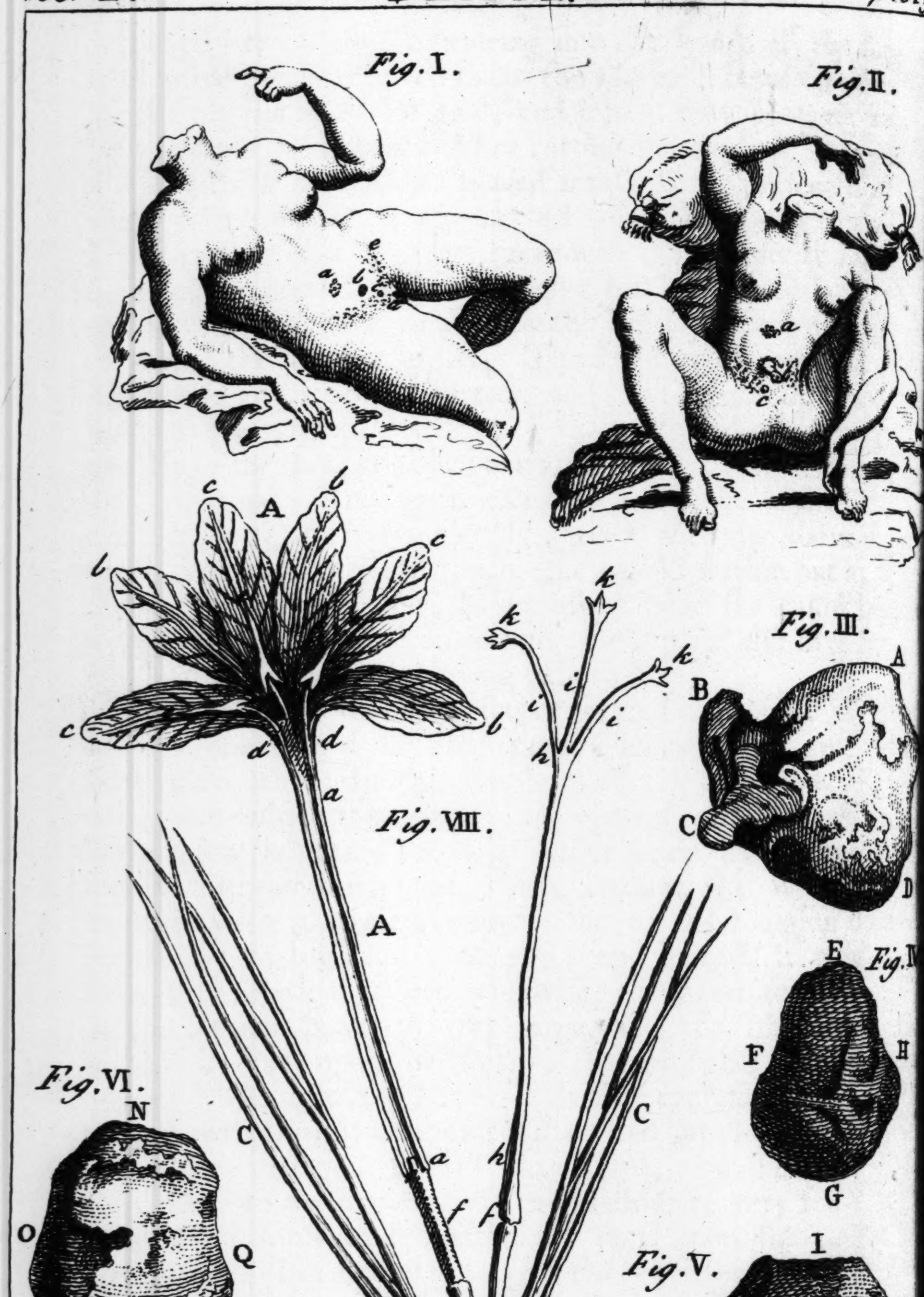
An *Account of the ſame* Woman; *by Dr.* Oliver. Phil. Tranſ. N° 379. p. 413. *Tranſlated from the* Latin.

THIS woman was formed in the following manner; a kind of fleſhy, ſpongy ſubſtance of a florid red colour, that could not bear the leaſt touch, without the moſt exquiſite pain, poſſeſſed the place of the navel, and ſprung, as it were, out of the cavity, which is generally in that part. From this excreſcence (which did not ſenſibly increaſe or diminiſh for ſeveral years) the urine continually ouz'd without having any other paſſage; nor could it by this with the ſtrongeſt effort be ſquirted out: A little below, at almoſt the diſtance of a ſmall palm, there was a ſort of paſſage, an inch in length and a finger in breadth, large enough to admit the forefinger and yet keep it tight. Her *menſes* flow'd by this paſſage; whence it plainly appeared, this was the orifice to the *vagina* and *uterus:* At almoſt the ſame diſtance, as the orifice of the *vagina* from the excreſcence, was another lower orifice, leſs than the upper, and ſcarce admitting the little finger: It was evident, that this orifice communicated with the *rectum*, from the thinneſt parts of the *fæces* being frequently voided thereby: The true *anus*, by which the harder *fæces* were diſcharged, was five or ſix inches diſtant from this falſe *anus*, and almoſt in the place where the *pudendum* naturally is. There were ſome few hairs, deſcribing two lines, which began from the middle ſpace between the excreſcence and the commiſſures of the thighs with the *abdomen*, and approaching each other formed an angle, in whoſe vertex the true *anus* was ſituated: She was afterwards married and proved with child; the orifice of the *vagina* was not at all dilated by coition; ſo that there aroſe ſeveral diſputes among the women about the manner of the impregnation. That the immiſſion of the *penis* is not abſolutely neceſſary in impregnation, we have ſeveral inſtances: However ſhe applied to a ſurgeon at *Foy*, for his advice, who, upon hearing the caſe and examining into the matter, promiſed his aſſiſtance, if neceſſary: Her time now drawing near, ſhe began to feel her pains and the other ſymptoms. The ſurgeon, being called, affirmed, that the *fœtus* made towards the region of the *pubis*; but ſhe herſelf, towards the orifice of the *vagina*. In ſhort, the ſurgeon inſiſted, that an inciſion muſt be made to open a paſſage for the child; this the by-ſtanders oppoſed, but her pains increaſing, ſhe was ſeiſed with convulſions and her life was deſpaired of. At length when all concluded ſhe was now near her end, they deli-

delivered her up almost expiring into the hands of the surgeon, who immediately resolved to cut the *isthmus*, between the orifice of the *vagina* and false *anus*, and which may properly enough be called the *perinæum*: After cutting the external integuments, a strong thick membrane, placed transversely, presented itself to view, which separated the *vagina* from the lower *canalicule*. Upon cutting this asunder, there was made a pretty large passage to the *uterus*, and introducing his hand he dilated the *os uteri*, laid hold of the child by the head, extracted it safe and recovered the mother to life. The Dr. himself went to *Foy* to know the truth of the matter, and the surgeon informed him both of the original structure of the parts and of the operation exactly in the manner above narrated, all which the woman herself confirm'd: And upon examining her, he found, that the excrescence at the navel exactly agreed with the above description; that upon the least touch, she would scream out as if run thro' with a sword; and being asked how she could bear the weight of her husband in coition, she replied the love she bore her husband made every thing easy to her.

But to return; this excrescence seemed to consist of several lobules, envelopp'd in their proper membranes, and distinct from each other: In the interstices of these lobules, the small pores thro' which the urine ouzed, opened at the superficies. A little below appeared the large orifice made by the surgeon for facilitating the birth; on which account she was ever after troubled with a *prolapsus uteri*, the *uterus* hanging down five inches and as big as a goose egg; and indeed, it would have been surprising had it been otherwise, the aperture at the orifice of the *vagina* being so very large, and the bladder pressing more than usual upon the *uterus*: It is probable, that the urine ascended thro' the *urachus* towards that excrescence, and originally flowed thro' it; because nature had provided no exit thro' the urinary passage: Yet it is impossible, that the urine should thus ascend to the navel unless the bladder be very full; and in that case it must needs continually press upon the *uterus* and force it out: The better to observe the conformation of the parts she was put in a reclining posture on the bed, and attempting to thrust up the *uterus*, it was no sooner done than the urine squirted out in several streams as thick as a hair thro' the pores of the excrescence to the distance of several feet, like so many jets: After the bladder was emptied, the *vagina* was easily replaced; and after the *vagina* and *uterus* were both returned there appeared a large chasm, three inches in diameter, whose

fimbri



J. Smith del. *Hulett Sculp*

fimbriæ rather than *labia*, had ſeveral unequal *ſarcoma*'s: In its upper internal margin the Dr. look'd (but in vain) for the urinary paſſage; in the lower margin he eaſily found the falſe *anus*, thro' which he introduced his finger under the *pubes* (which ſeemed to approach to a cartilaginous ſoftneſs) into the *rectum*; whence he ſuppoſes the *rectum* perforated three inches above the *ſphincter* of the *anus*; a little below the *pubes*, the true *anus* was ſituated, in the place where the *ſinus* of the *pudenda* is in other women.

a Fig. 1. Plate X. the umbilical excreſcence; *b* the orifice of the *vagina*; *c* the falſe *anus*; *d* the *anus*; *e e* the hairs.

a Fig. 2. the umbilical excreſcence; *b* the *os uteri*, *c* the *anus*.

A ſurpriſing Flux of Blood *thro' the* Penis; *by Dr.* Howman. Phil. Tranſ. N° 379. p. 418. *Tranſlated from the* Latin.

A Citizen of *Norwich*, 40 years of age, in perfect health, was ſeiſed the 30th of *June* 1716 with a flux of pure blood thro' the *urethra*; the ſame phænomenon returned the 31. of *July*: By the application of ſome medicines and drawing blood in the arm, the flux was ſtopped, till the 8. of *September* and afterwards it entirely ceaſed: It is remarkable, that the abovementioned flux happened without any previous pain, or ſubſequent dejection of ſpirits.

An Account of the Pits *for* Fullers-earth *in* Bedfordſhire; *by Mr.* Holloway. Phil. Tranſ. N° 379. p. 419.

MR. *Holloway* went to the fullers-earth pits at *Wavendon* near *Woburn* in *Bedfordſhire*, where there are ſeveral of them open; and as the men were then at work only in one and underſtanding that the earth was diſpos'd in much the ſame manner in all, he went down in no more than that wherein they were then digging, where he found things diſpoſed in the following manner.

From the ſurface, for about ſix yards depth, there are ſeveral layers of ſand, all reddiſh, but ſome lighter coloured than others, under which there is a thin *ſtratum* of red ſand-ſtone, which the workmen break thro'; and then for the depth of about ſeven or eight yards more, you have ſand again, and after that you come to the fullers-earth; the upper layer of which, being about a foot deep, the diggers call the *cledge*, which is thrown by as uſeleſs, by reaſon of its too great admixture with

the neighbouring ſand, which covers and has inſinuated itſelf into it; after this they dig up earth for uſe, to the depth of about eight feet more; the matter of which is diſtinguiſhed into ſeveral layers; there being commonly about a foot and a half between one horizontal fiſſure and another: The upper half of theſe layers of fullers-earth, where the earth breaks of itſelf, is tinged red, as it ſeems by the running of water from the ſandy *ſtrata* above; and this part they call the *crop*; between which and the *cledge* abovementioned, there is a thin layer of matter, not an inch in depth; in taſte, colour and conſiſtence not unlike *terra Japonica*; the lower half of the layers of fuller's-earth, they call the *earth-wall*; this is not tinged with the abovementioned red colour, and ſeems to be the more pure and the fitter for *fulling*; and underneath all is a layer of white rough ſtone, of about two foot in thickneſs; which, if they dig thro', as they very ſeldom do, they find ſand again, and then there's an end of their works.

There is one thing obſervable in the ſite of this earth, *viz.* that it ſeems to have every where a pretty equal horizontal level; becauſe they ſay, that when the ſand-ridges at the ſurface are higher, the fullers-earth lies proportionably deeper.

In theſe works they ſeldom undermine the ground; but as they dig away the earth below, others are employed to dig and carry away the ſurface; otherwiſe, the matter above, being of ſo light and flitting a nature, would fall in and endanger the workmen: For, as was obſerved above, that *ſtratum* of ſand-ſtone, which occurs before they come to the fuller's-earth, does not lie, as in coal-pits, immediately over the matter they dig for, like a cieling; but even in the middle of the ſuperjacent *ſtrata* of ſand, and therefore can be no ſecurity to them, if they undermine.

The perpendicular fiſſures are frequent, and the earth in the *ſtrata*, beſides its apparent diſtinction into layers, like all other kinds of matter, by reaſon of its peculiar unctuouſneſs, or the running of the adjacent ſand imperceptibly into it, breaks into pieces of different angles and ſizes.

As to the geographical ſituation of theſe pits, they are dug in that ridge of ſand-hills by *Woburn*, which near *Oxford* is called *Shotover*, on which lies *Newmarket-Heath* by *Cambridge*, and which extends itſelf from eaſt to weſt, every where, at about the diſtance of eight or ten miles from the *Chiltern* hills, which in *Cambridgeſhire* are called *Gog-Magog*; in *Bucks* and *Oxon*, the *Chiltern* hills, from the chalky matter of which

they

they chiefly consist; which two ridges you always pass, in going from *London* to the north, north-east, or north-west counties in the manner above-mentioned: After which you come into that vast vale, which makes the greater part of the midland counties of *Cambridge*, *Bedford*, *Bucks*, *Northampton*, *Oxford* and *Gloucester*; and in which are the rivers *Cam*, *Ouse*, *Nen*, *Avon*, *Isis* and others: Which Mr. *Holloway* takes notice of, because it confirms what Dr. *Woodward* says of the regular disposition of the earth into like *strata* or layers of matter, commonly thro' vast tracts; and hence Mr. *Holloway* questions, whether fullers-earth may not probably, be found in other parts of the same ridge of sand-hills, among other like matter.

An Invitation for making Meteorological Observations; *by Dr.* Jurin. Phil. Transf. N° 379. p. 422. *Translated from the* Latin.

THE various temperaments of the air (we breathe in) as to heat and cold, or the various changes of moist and dry weather, especially if great and sudden, are justly reckoned to influence more or less the health of mankind: In making these observations, therefore, not only physicians, but other curious enquirers into nature have in all ages been at no small pains: And in the last century, by the diligence and ingenuity of philosophers; instruments and machines were invented, by which the several degrees and changes of the weight, heat, moisture and elasticity of the air, are not only exhibited to the eye, but brought to weight and measure.

Nor did they stop here, but they endeavoured to discover, as much as possible, the causes of these changes; for which purpose they carefully set down in diaries the observations, made with these new invented instruments, on the weight, moisture and heat of the circumambient air, and to these they added several other observations, with respect to the weather, and face of the sky, the winds and quantity of rain, as may be seen in the *Philosophical Transactions* and elsewhere.

Perhaps it is not easy finding a better method of observation, than that just now mentioned; and were there a competent number of observers stationed in convenient places; and should some one compare all their diaries together to observe wherein

wherein they agreed or differ'd from each other, we ſhould then have ſuch a hiſtory of the air, as now we can hardly expect.

For, it is certain, that generally the ſudden changes of the weather are chiefly to be attributed to the winds; and when we come to know (by ſuch a method of obſervation, as that above-mentioned) in what places they roſe, what courſe they kept, at what time, and thro' what tracts of the earth they paſſed over; by this means, probably, a way might be opened for diſcovering the origin and cauſes of the winds: This one thing at leaſt, (which would be of no ſmall conſequence in theſe diſquiſitions, and which now is generally reckoned a probable conjecture) would be gain'd by it, *viz.* we ſhould be enabled by accurate obſervations to find out either the truth or falſhood of Dr. *Halley*'s opinion in *Phil. Tranſ.* N° 181, who thinks that the aſcent of the mercury in the barometer is owing to the winds blowing towards the ſame place from contrary points, and ſo collecting the air and accumulating it, as it were; as on the contrary that its deſcent is cauſed by the winds carrying the air from the ſame place towards contrary parts, and thus exhauſting it, as it were.

The curious, therefore, (who would contribute to improve this part of natural hiſtory) are deſir'd to mark in their diary, once a day at leaſt, the height of the barometer and thermometer, the point the wind blows from, with ſome account of its degree of ſtrength, the face of the heavens and the quantity of rain or ſnow, fallen ſince the laſt obſervation: And ſhould any one be pleas'd to add his obſervations, made with a hygroſcope or with a magnetical needle, it would not be at all unacceptable.

It would be proper as often as any violent ſtorm happens, accurately to ſet down its riſe, increaſe, greateſt violence, its abating and period, as alſo the reſpective heights of the barometer.

Such as know the method of filling and making a barometer, may uſe the common or open one, as it is call'd; let the tube be the fourth at leaſt, or the third part of an inch in diameter, ſince the mercury is found to ſubſide in narrower tubes below its juſt height, vide *Phil. Tranſ.* N° 363, let the diameter of the ciſtern or veſſel containing the mercury be at leaſt 8 or 10 parts greater than the diameter of the tube;

tube; that when the mercury rises or falls in the tube, its height in the ciſtern may continue invariable, or as nearly ſo as poſſible.

Such as chuſe to make uſe of a cloſe or portable barometer, may have them made by that ingenious artiſt Mr. *Francis Hauksbee* in *Crane-court*, *London*; who can furniſh them with accurate thermometers, graduated according to a ſcale, for many years known to the curious.

Such as make uſe of any other thermometer, are deſir'd to mark in their diary its ſite, ſtructure, the ſcale it is graduated by, as alſo the name of the workman: The Dr. takes the moſt convenient place for a thermometer to be a room towards the north, where a fire is never or at leaſt very rarely made.

That the diaries may be the more eaſily compared together, it will be proper to obſerve the following method.

Let the firſt column exhibit the day and hour of the obſervation: All the obſervers are deſir'd to make uſe of the *Julian* or old ſtile in their diaries.

The ſecond the height, to which the mercury riſes in the tube above the ſurface of that in the ciſtern, in inches (or the twelfth part of a *London* foot) and decimal parts of an inch; a *London* to a *Paris* foot is as 15 to 16 nearly.

The third, the degree, and the decimal parts of a degree, to which the ſpirits in the thermometer reach.

The fourth, the point the wind blows from and its degree of ſtrength, which laſt may always be ſhewn by ſome of the following numbers, 1, 2, 3, 4; 1 denoting the ſofteſt breath of air, that ſcarce moves the leaves of trees; 4 its greateſt degree of ſtrength; 2 and 3 the intermediate degrees thereof; and 0 a perfect calm.

The 5th, the face of the heavens, with a ſhort account of the weather.

The 6th and laſt, the height of rain or diſſolv'd ſnow, fallen ſince the laſt obſervation, in *London* inches and decimal parts of an inch.

This may be eaſily eſtimated by means of a funnel, two or three foot wide, another veſſel receiving the water from the funnel, and a cylindrical meaſure with a gage divided into inches and decimal parts of an inch: Let the ſituation of the funnel be ſuch, that whatever wind blows, no part of the rain may be intercepted, either by the intervention of the houſe or any other impediment. Let the veſſel containing the

the water be close shut every way that none of it may evaporate, with only a narrow hole to receive the water from the funnel above: Moreover, let the diameter of the cylindrical measure be 10 parts less than that of the funnel; so that by this means an inch of water in the measure may be estimated to have fallen to the height of the hundredth part of an inch in the funnel, and consequently, on the rest of the earth; and in like manner for decimal parts of an inch.

At the end of every month and year let the mean height of the barometer and thermometer in each be subjoined; as also the sum of all the heights of the rain, fallen in the whole month or year: The said mean height will be obtained, by bringing into one sum all the observations of the heights of the barometer, made in the morning, and the observations of the thermometer either in the morning or the greatest that day (which happens about three or four o'clock in the afternoon) and dividing that sum by the number of days.

All, who are pleased to make the abovementioned observations, either in whole or in part, are desired to transmit copies of the diaries, continued to the end of each year, to the secretaries of the *Royal Society*; to the end they may be compared with the diary, made at *London*, by the said *Society*'s order; and it is proposed, that whatever can be gathered from comparing these diaries together, shall be published every year in the *Philosophical Transactions*.

The

The form of the diary

Day and hour		Height Barom.	Height Therm.	winds	Weather	Rain
1723 Nov. O.	S.	inches tenths	deg. tenths			inches tenths
1. 8	*a. m.*	29 . 75	49 . 6	S. W. 1	The sky overcast	0 . 035
4	*p. m.*	29 . 56	47 . 3	S. W. 2	Showers at intervals Sunshine at times	0 . 043
2. 7 ½	*a. m.*	29 . 24	48 . 5	S. 1	Almost constant rain	0 . 725
3. 9	*a. m*	29 . 95	49 . 7	N. 1	Clear sky	0 . 032
5	*p. m.*	30 . 4	49 . 2	N. 1	Clear sky	0 . 000
4. 7	*a. m.*	29 . 9	47 . 0	S. W. 1	Scattered clouds	0 . 000
10		29 . 7	46 . 2	S. W. 2	Showers at times	0 . 103
12		29 . 4	45 . 0	S. 3.	The sky almost quite covered with clouds	0 . 050
3	*p. m.*	28 . 8	46 . 0	S. 4	Scattered clouds	0 . 000
5		28 . 6	47 . 2	S. W. 4	The same face of the sky	0 . 000
7		28 . 9	48 . 0	S. W. 2	Rain	0 . 000
9		28 . 9	48 . 2	0	Almost constant rain	0 . 305
5. 7	*a. m.*	29 . 7	53 . 4	N. E. 1	Clear sky frost	0 . 250

An Account of Stones *voided* per anum; *by Mr.* David Martineau. Phil. Trans. N° 380. p. 433.

THE stones, represented by Fig. 3, 4, 5, 6, 7. Plate X. were voided by a woman by the *anus*, on the 26, 27 and 28. of *March* 1723, who was then, according to computation, about the 11th or 12th week gone with child. *March* 23. Mr. *Martineau* was called in the evening to this woman, who in appearance was in the extremity of a convulsion fit, attended with violent vomitings, after which, she complained of great pain in her back, from her reins downwards to the *anus*; upon which he blooded her, order'd some anti-emetics and left her: She continued in great pain all the day; on the 24. in the evening her fits returned again with double force, her pains also increasing like labour pains, put Mr. *Martineau* upon farther enquiry, but nothing appeared more than ordinary: The extremity of pain the patient was in put him on using glysters; but none could be thrown up; upon which he gave her a gentle draught, which she observ'd increased her pain with a strong *tenesmus*, that continued near three hours, before the largest stone Fig. 3. appeared, which stopped at the upper part AD, but was quickly removed by pressing the lower protuberance C; upon which, with a plentiful discharge, she had ease, slept some hours, waking with a desire to go to stool, and with it she voided two more, represented Fig. 4, 5; on the next day at 4 hours interval the other 2, represented Fig. 6, 7. The patient recovered perfectly, and on the 24th of *August* was delivered of a living female child. Upon enquiry, Mr. *Martineau* found, she had been frequently troubled for 14 years, with pains in her side and stomach without vomitings.

The largeness of the stones, their seat and substance, is what seemed remarkable to Mr. *Martineau*; they were all alike in colour and weight, according to their dimensions.

	oz.	*p.w.*	*gr.*		*inch.*			*inch.*
Fig. 3.	2	16	22	ABCD	8	AD	the top of the	6
Fig. 4.		8	12	EFGH	$5\frac{1}{8}$	EG	stone oval.	4
Fig. 5.		7	3	IKLM	5	IL		3
Fig. 6.		7	12	NOPQ	4 8	NP		3
Fig. 7.		5	12	RSTU	$4\frac{3}{8}$	RT		4

In the last figure 1 represents a crust taken off the stone at the extremity; 2 the stone and crust soft like a chesnut and resembling wool; upon cutting into the stone at 3, it was found hard.

Of the Globules *in the* Blood *and* Lees *of* Wine; *by M.* Leewenhoeck. Phil. Trans. N° 380. p. 436. *Translated from the* Latin.

BY a letter from Dr. *Jurin* to M. *Leewenhoeck*, it appears that the former found his observations about the size of the blood-globules agreeing with those made by the latter; and moreover, that he thought this disquisition not indifferent or useless; since it was not impossible, but that such an observation of the size of the blood-globules in different animals, together with their small vessels, might some time or other lead us to find out where and by what means these globules are form'd, and hence retain a determinate thickness in the same animal: Last of all, the Dr. says, that this well deserved farther enquiry.

To this M. *Leewenhoeck* objects, that he is apt to think, we shall never come to the knowledge how these globules are coagulated from a fluid matter into other globules of a determinate bigness.

M. *Leewenhoeck* procured wine on the fret, because it produces globules, almost equal to those of the blood, and which we call the lees; and tho' he viewed this wine thro' a microscope, yet he observed nothing other than a vast number of exceeding small air-globules, rising up to the surface, and carrying along with them those globules, called the lees; as soon as they reached the surface of the wine, they were separated from the air-globules and subsided, without being able to discover, how the globules, we call the lees, were formed: and as soon as these globules subsided, another air-globule proceeded from each of them, that mounted up again to the surface.

M. *Leewenhoeck* imagines, that that coagulation and determinate size of the blood-globules was originally so: For, if one globule were bigger than another, the consequence would be an irregular circulation; because he had observed the blood flow with rapidity thro' various blood-vessels so fine, as only to admit a single globule to pass thro' them at a time.

Of the Generation of Animals *and of the Palpitation of the* Diaphragm; *by M.* Leewenhoeck. Phil. Tranſ. N° 380. p. 438. *Tranſlated from the* Latin.

DR. *Jurin* in a letter to M. *Leewenhoeck* acquainted him, that ſome anatomiſts attempted to write a new ſyſtem upon his notion of generation, affirming, that the *ovum* is the proper *nidus* in which the *animalculum* of the *ſemen maſculinum* is lodged, and that the ſame *ovum* being thereby impregnated, is afterwards conveyed thro' the *tuba Fallopiana* from the *ovarium* into the *uterus*.

M. *Leibnitz* a little before his death, wrote a letter to M. *Leewenhoeck* from *Hanover*, acquainting him, that he embraced his opinion on generation, and demonſtrated it in a book, he publiſhed; but that a Gentleman in *Italy* was about publiſhing ſomething againſt this ſyſtem of generation; and that another in *Germany* told him, that the whole *ovarium* was found in the *tuba Fallopiana*; and that there were profeſſors in *Germany*, who denied the exiſtence of any *animalcula* in the *ſemen maſculinum*.

Yet notwithſtanding all the contradictions M. *Leewenhoeck* had already or would hereafter ſuffer, he propoſed to continue in his opinion about generation; becauſe, he found *animalcula* in all ſorts of ſeeds, not excepting the various ſpecies of birds and fiſhes.

Moreover, we find in all the ſeeds of trees and plants, even the ſmalleſt (provided they are not impalpable) that the plant is formed, and which he formerly compared with the *animalcula* in the *ſemen maſculinum*; and the mealy matter in ſeeds is deſtined for nouriſhing theſe little plants within them, till itſelf throws out its roots and can be nouriſhed from the earth.

M. *Leewenhoeck* could not conceive, how any ſhould attempt to lay down and publiſh ſuch idle poſitions, *viz.* that the *animalcula* of the *ſemen maſculinum*, laid in the *uterus*, or *tuba Fallopiana* ſhould afterwards be conveyed thro' the *tuba Fallopiana* from the *ovarium* into the *uterus*. Theſe poſitions are ſo much without any foundation, as to deſerve no farther anſwer.

M. *Leewenhoeck* was perſuaded that the diaphragm is ſo large an inſtrument, as to put the whole *abdomen* into a continual motion, whereby the food in the ſtomach and inteſtines is comminuted in ſuch a manner, as to be reduced into a fluid

fluid matter, fit to enter theſe ſmall blood-veſſels, ſo numerous in the cavities of the inteſtines, not as ſome would perſuade us, thro' the orifices; but as M. *Leewenhoeck* thinks thro' the fine coats of theſe exceeding ſmall blood-veſſels.

Now, if we ſuppoſe, that an artery in a well conſtituted body has 3600 pulſations in an hour, and that four pulſations are performed in the time of one reſpiration, there will thence be computed 900 reſpirations every hour, and ſo often will the ſtomach and inteſtines be compreſſed.

M. *Leewenhoeck* felt ſeveral ſlow palpitations in his diaphragm; wherefore, he thought of a glaſs-tube, he had invented, to convey the ſpirits of a certain liquor into his breaſt, in order to try, whether that uncommon motion of the diaphragm could be removed.

He therefore filled the tube to the height of three fingers with ſpirits of wine, in which were infuſed for a long time the following aromatics, *viz.* nutmeg, mace, a little clove, a deal of cinnamon and ſaffron; he then ſtrongly ſuck'd into his lungs the air that paſſed thro' the ſpirits of wine, by which means he expanded his diaphragm (which is a ſtrong membrane) pretty much, in order to try, whether he could thereby lay the palpitation or uncommon motion of the diaphragm; which ſucceeded for that time: But afterwards it happened, that this experiment (tho' he had drawn in the air very ſtrongly) did not anſwer his expectation.

M. *Leewenhoeck* rather inclines to Dr. *Jurin*'s opinion, *viz.* that the palpitation of the diaphragm is better grounded than that of the heart.

A Botanical Deſcription of the Flower and Seed Veſſels of the Plant, called Crocus autumnalis ſativus, *that produces the true* Engliſh Saffron *of the Shops; by Dr.* James. Douglas. Phil. Tranſ. N° 380. p. 441.

THE flower of this curious plant is of the lilly kind, monopetalous, *infundibuliformis*, without any *calyx* or *perianthium*; its long fiſtulous beginning being afterwards expanded into ſix beautiful oblong ſegments, as repreſented at A A Fig. 8. Plate X.

It conſiſts of the following parts, *viz.* the *petalum*, the *ſtamina*, the *apices*, the *ovarium*, and the *ſtylus*, with its *capillamenta tubiformia*.

The *petalum* is diſtinguiſhed into a long, hollow or fiſtulous part, which lies incloſed within the common and proper

involucra

involucra of the plant, arising from the top of the *ovarium* or seed vessel, and six segments, as represented at *a a*.

While this narrow, tubulous part runs between the leaves and integuments that surround them, it is of a white colour; but as soon as it is disengaged from these, it insensibly acquires a purple colour, which, a little before its division, as it begins to be enlarged, and grow more open, inclines to a red.

This narrow fistulous part of the flower, about 1 or 2 inches above the *theca communis*, forms six foliaceous segments, or divides into so many oblong, purple coloured *petala*, as represented *b c*.

Three of these flower-leaves are larger than the other three; but in all other respects are much alike, as at *c c c*.

The length of the largest is from 1 inch $\frac{3}{4}$, to two inches; the breadth seldom above $\frac{1}{2}$ an inch: The shorter leaves are from 1 inch, to 1 inch and $\frac{3}{4}$ in length; their breadth being something less in proportion.

The inside of each *petalum* is of a violet-purple colour, vein'd with a few small lines, of a deeper dye, running lengthways, intermix'd with white; or the whole is beautifully checquered with blue and white colours.

The outside is of a lighter blue, with several whitish risings or ridges; and just at the bottom of the leaf it is of a deeper blue: The three small leaves *b b b*, are much of the same colour, only the purple seems to be somewhat deeper.

The number of the *petala* is for the most part 6: Yet in some flowers we may observe 7 or 8, but then they are not so large, as when they are fewer.

In each flower are three *stamina* or chives, *d d d*, properly so called, arising from the inner surface of the tubular part of the flower, just before its division into the *petala*, where they make, for some space, an appparent ridge; and then they stand upright, opposite to the three large leaves; they are of a whitish colour, inclining to a light purple, being but little above $\frac{1}{4}$ of an inch in length.

In those plants that have seven or more *petala*, the number of the *stamina* is likewise increased to five or more.

Each of these *stamina* has its proper *apex*, *e e e*, which is a peculiar oblong substance, standing upright, opposite to the larger flower-leaves, of a yellow colour, fork'd, at the lower end, where it is not unlike the tongue of a small bird, fixed to the *stamen*; here likewise it is broadest; but as it ascends

ascends it becomes narrower and its upper extremity is a little crooked, or turned to one side, it seldom exceeds $\frac{1}{2}$ an inch in length.

It appears as if it were double with a longitudinal furrow in the middle, in which hollow the *stamen* seems to be faintly continued for some space.

When the plant is full grown, they are all loaded with that kind of small dust, called *farina fœcundans*.

The *ovarium*, called likewise the *vasculum seminale* and *pistillum* by some, as *ff*, is a particular body, which arises from the top of the *pedunculus g*, about $\frac{1}{4}$ of an inch long or a little more, of a deep white colour, three cornered and divided into three *loculamenta* or *capsulæ*, in which the seeds (which however seldom come to perfection with us) are form'd, growing bigger and bigger, after the flower falls off; nay, even in this blooming state, if you cut this vessel a-cross the middle, you may perceive the whitish rudiments of the seeds.

From the upper part or *apex* of the *ovarium* arises the *stylus h*, a long slender tube, that lies inclosed within the tubulous or fistular portion of the flower, being there of a whitish colour, but changing into a yellow before its division.

This *stylus* ordinarily splits into three parts, just opposite to the top of the *stamina*, where the *apices* take their rise *i i i*, and thus far it stands upright in the middle of the *stamina*; for the sake of which only, this plant is cultivated, and being prepared (as shall be described anon) makes the true saffron of the shops, so frequently used in physic.

The Dr. calls these parts of the *stylus*, from their figure and shape, *capillamenta tubiformia* or *appendices styli salpingoeides*, in as much as they exactly represent a trumpet, and are not unlike the *tuba Fallopiana* in women, being narrower at their origin and growing gradually larger towards the other extremity; which, like the common trumpet, is open and expanded; and like the *tubæ Fallopianæ*, is jagged or fringed at the edge, as *k k k*: It may likewise be very properly called *crocus officinarum*, because that is the only part that is used in the shops.

They are of a yellow colour, just at their beginning, continued from the upper part of the *stylus*, but afterwards they are all of a deep red colour, only their jagged extremities are tipt with a white, inclining to a yellow.

Those *tubæ* or *capillamenta* are from 1 to 1 inch and $\frac{1}{2}$ in length.

The

The *stylus* while undivided, is strong enough to support itself, being inclosed within the tubular part of the flower; but the *capillamenta*, being very weak and slender at their beginning hang down between the *petala*.

Fig. 8. Plate X. represents a root of the saffron plant, that has two stalks inclosed in one common *vagina*; the whole flower with the *stamina* and *apices* in one, and the *stylus* only in the other; with the leaves, *pedunculi* and *ovarium* in both being fairly delineated from the life; A A the flower expanded into six beautiful segments; *a a* the fistulous part of the flower; *b c* the six *petala*; *c c c* the three larger *petala*; *b b b* the three smaller ones; *d d* the three *stamina* or chives; *e e e* the three *apices*; *f f* the *vasculum seminale*; *g g* the *pedunculi*; *h h* the *stylus*; *i i i* the three *capillamenta tubiformia*; *k k k* the jagged extremity of the *capillamenta*; B B the root; C C the grassy leaves.

An Account of M. Leewenhoeck's *curious* Microscopes, *presented to the* Royal Society; *by Mr.* Folkes. Phil. Trans. N° 380. p. 446.

IT is upwards of 50 years, since the late M. *Leewenhoeck* first began his correspondence with the *Royal Society*; when he was recommended by Dr. *Regnerus de Graaf*, as a person already considerable by his microscopical discoveries, made with glasses, contrived by himself, and excelling even those of the famous *Eustachio Divini*, so much talked of in the learned world; and as he had ever since that time applied himself, with the greatest diligence and success, to the same sort of observations, no doubt can be made of the excellency of those instruments he had so long used, so much improved, and upon the fullest experience so often commended in his letters; a great part of which, at his decease, he bequeathed to the *Royal Society*.

This legacy consists of a small *Indian* cabinet, in the drawers of which are 13 little boxes or cases, each containing two microscopes, handsomely fitted up in silver; all which, not only the glasses, but also the *apparatus* for managing of them were made with M. *Leewenhoeck*'s own hands: Besides which they seem to have been put in order in the cabinet by himself as he designed them to be presented to the *Royal Society*, each microscope having had an object placed before it, and the whole being accompanied with a register of the same in his own hand writing; as being desirous the Gentlemen of the *Society* should

without

without trouble, be enabled to examine many of those objects on which he had made the most considerable discoveries, and which in a particular manner deserve their attention.

The 13 cases abovementioned are numbered from 15 to 27 inclusive, corresponding to which is the register of the objects, two to every case, as follows.

N° 15. Globules of blood, from which its redness proceeds.
A thin slice of lime-tree wood, where the vessels, conveying the sap, are cut transversely.

N° 16.
The eye of a gnat.

N° 17. A crooked hair, to which adheres a ring-worm, with a piece of the *cuticula*.
A small hair from the hand, by which it appears, that those hairs are not round.

N° 18. A piece of the cod-fish (*cabeljaeuw*) shewing how the fibres lie oblique to the membranes.
An embryo of cochineal taken from the egg, in which the limbs and horns are plainly to be seen.

N° 19. Small pipes, which compose the tooth of an elephant.
Part of the crystalline humour from the eye of a whale.

N° 20. A thread of sheep's wool, which is broken and seems to consist of several lesser threads.
The instrument, whence a spider spins the threads, that compose his web.

N° 21. A granade or spark made in striking fire.
The vessels in a leaf of tea.

N° 22. The *animalcula in semine masculino* of a lamb, taken from the testicle *July* 24, 1702.
A piece of the tongue of a hog, full of sharp points.

N° 23. A fibre of codfish, consisting of long slender particles.
Another of the same.

N° 24. A filament, conveying nourishment to the nutmeg, cut transversely.
Another piece of the same, in which may be seen the figure of the vessels.

N° 25. Part of the abovementioned bone or tooth, consisting of hollow pipes.
An exceeding thin membrane, which covered a very small muscle.

N° 26. Veſſels, by which membranes receive nouriſhment and increaſe.

A bunch of hair from the inſect, called a hair-worm.

N° 27. The double ſilk, ſpun by the worm.

The eye of a fly.

It were endleſs to enter into any particulars of what is to be obſerved in any of theſe objects or give any account of M. *Leewenhoeck*'s diſcoveries; they are ſo numerous as to make up a conſiderable part of the *Philoſophical Tranſactions*, and when collected together to fill four pretty large volumes in quarto, which have been publiſhed by himſelf at ſeveral times: And of ſuch conſequence, as to have opened entirely new ſcenes in ſome parts of *Natural Philoſophy*, particularly, that famous diſcovery of the *animalcula in ſemine maſculino*, which has given a perfectly new turn to the theory of generation, in almoſt all the authors that have ſince wrote upon that ſubject.

As to the conſtruction of theſe inſtruments; it is the ſame in them all, and the *apparatus* is very ſimple and convenient: They are all ſingle microſcopes, conſiſting each of a very ſmall double convex glaſs, let into a ſocket, between two ſilver plates, rivetted together, and pierced with a ſmall hole; the object is placed on a ſilver point or needle, which by means of ſcrews of the ſame metal, provided for that purpoſe, may be turned about, raiſed or depreſſed, and brought nearer, or put farther from the glaſs, as the eye of the obſerver, the nature of the object, and the convenient examination of its ſeveral parts may require.

M. *Leewenhoeck* fixed his objects, if they were ſolid, to this ſilver point, with glue; and when fluid, or of ſuch a nature, as not to be commodiouſly viewed, unleſs ſpread upon glaſs, he firſt fitted them on a little plate of talc, or exceeding thin blown glaſs, which he afterwards glewed to the needle, in the ſame manner as his other objects.

It is true, the obſervation of the circulation of the blood and ſome others, require a ſomewhat different *apparatus*; and ſuch a one he had, to which he occaſionally fixed theſe ſame microſcopes: But as it makes no part of this cabinet, Mr. *Folkes* only takes notice, that it may be ſeen in a letter to the *Royal Society*, dated the 12. of *January*, 1689. and publiſhed in his *Arcana naturæ detecta* N° 69. and this may ſerve to ſhew the univerſal uſe of theſe microſcopes, and (among other things) induced

induced Mr. *Folkes* to believe, these were the kind of microscopes, generally, if not solely, used by this curious Gentleman in all his observations, and to which we are obliged for his most surprising discoveries.

Upon the late Queen *Mary*'s visiting M. *Leewenhoeck* at *Delft*, and viewing his curiosities with great satisfaction, he presented her with a couple of his microscopes, which, as Mr. *Folkes* was informed by one who had them a considerable time in his custody, were of the same sort with these, and did no ways differ from any of the 13 cases, contained in the drawers of this cabinet.

The glasses are all exceeding clear, and shew the object very bright and distinct, which must be owing to the great care this Gentleman took, in the choice of his glass, his exactness in giving it the true figure; and afterwards, amongst several, reserving only such for his own use, as he, upon trial, found to be most excellent. Their powers of magnifying are different, as different sorts of objects may require; and as on the one hand, being all ground glasses, none of them are so small; and consequently, magnify to so great a degree, as some of those drops, frequently used in other microscopes: Yet on the other hand, the distinctness of these very much exceeds what Mr. *Folkes* had met with in the glasses of that sort; and this was what M. *Leewenhoeck* ever principally proposed to himself, rejecting all those degrees of magnifying, in which he could not so well obtain that end: For, he informs the *Royal Society* in one of his letters to them, where speaking of the excessive praise some give their glasses upon this account, that tho' he had had upwards of 40 years glasses by him of an extraordinary smallness, he had made but very little use of them; as having found, in a long course of experience, that the most considerable discoveries were to be made with such glasses, as magnifying but moderately, exhibited the object with the most perfect brightness and distinctness.

But however excellent these glasses may be judged, M. *Leewenhoeck*'s discoveries are not entirely to be attributed to their goodness only; his own great judgment and experience in the manner of using them; together with the continual application, and indefatigable industry with which he often and long viewed the same subject, and that under many and different circumstances, cannot but have enabled him to form better judgments of the nature of his objects, and see farther into their constitu-

 tion,

tion, than it can be imagined any other perſon can do, that neither had the experience, nor had taken the pains this curious author had ſo long done.

Nor ought we to forget a piece of ſkill, in which he very particularly excelled, which was that of preparing his objects in the beſt manner, to be viewed by the microſcope; and of this any one will be ſatisfied, who ſhall apply himſelf to the examination of ſome of the ſame objects, as do ſtill remain before theſe glaſſes; at leaſt Mr. *Folkes* found ſo much difficulty in this particular, as to obſerve a very ſenſible difference between the appearances of the ſame object, when applied by himſelf and when prepared by M. *Leewenhoeck*, tho' viewed with glaſſes of the very ſame goodneſs.

Mr. *Folkes* the rather inſiſts on this, as it may be a caution, not raſhly to condemn any of M. *Leewenhoeck*'s obſervations, tho' even with his own glaſſes, we ſhould not immediately be able to verify them ourſelves. We muſt be under very great diſadvantages for want of the experience he had; and M. *Leewenhoeck* himſelf has put us in mind more than once, that ſuch as are the beſt ſkilled in the uſe of magnifying glaſſes may be miſled, if they give too ſudden a judgment upon what they ſee, or till they have been aſſured from repeated experiments. But we have ſeen ſo many and thoſe of his moſt ſurpriſing diſcoveries ſo perfectly confirmed, by great numbers of the moſt curious and judicious obſervers, that there ſurely can be no reaſon to diſtruſt his accuracy in thoſe others, which have not hitherto been ſo frequently or carefully examined.

The Bills of Mortality, *&c.* of *several considerable Towns in* Europe, *from* Christmas 1716, *to* Christmas 1717, *extracted from the* Acta Breslaviensia; *by Dr.* Sprengell. Phil. Transf. N° 380. p. 454.

Breslaw in *Silesia.*

Buried		Christened	
In *January*	91	Males	584
February	113	Females	576
March	136		
April	108		1160
May	127	Among the Buried were.	
June	144	Married men	226
July	144	Married women	144
August	208	Widows and widowers	157
September	128	Batchelors	60
October	96	Maidens	57
November	113	Boys } under ten	419
December, only till		and girls } years old,	397
the 24th,	77	Still-born { Boys	37
		Still-born { Girls	17
	1485		1514

N. B. Here in the sum total are 29 more than in the number of the buried above, by reason of several still-born children that had no regular burial, and so were not reckoned.

In the imperial town *Vienna* there died.

Young people	3179
Old	2026
Total	5205

Among which were,

Of 90 years old	2	Of 99	2
91	1	100	1
92	3	102	2
93	1	103	1
94	3	104	1
95	2	115	1
96	3		

Born and christened 4030 children.

In *Dresden, Saxony,* died 1908
Christened 1443

In

In *Dantzick* died		1605
	Christened	2102

In *Esperies*, or *Eperies*, alias *Epperies*, a town in *Upper Hungary*, died — 132
Christened — 157
Most of these died of the small-pox

The Bills *of* Mortality *from* Christmas 1717 *to* Christmas 1718.

Breslaw.

Buried.	
From the 25th of *Dec.* to the 31st.	21
January	129
February	112
March	108
April	98
May	88
June	79
July	86
August	118
September	98
October	119
November	100
December, till the 24th,	81
Total	1255

Christened.	
Males	59
Females	55
	115

Among the dead were.		
Married men		23
——— Women		14
Widows and widowers		12
Batchelors		60
Virgins		6
Children to 10 years of age,	Boys	28
	Girls	28
Stillborn	Boys	4
	Girls	2
		125

In *Vienna* died	Men	1432	Women	1129
	Boys	1844	Girls	1705

In all 6110

Among which were froze to death in the street	2
Murder'd	2
Kill'd by a waggon	2
By falls from houses, *&c.*	11
Kill'd with swords, knives, and sabres,	9
By taking wrong physick	1
Trod to death by horses	4
Drowned	
Beat to death	
Smother'd in a house office	
Cut their throats	
Kill'd by the fall of houses	

Among

Among the dead were found

8 Persons of	90	years old.
7	92	
2	93	
3	94	
3	95	
1	96	
2	97	
2	98	
2	99	
1	105	

Christened

Males	2185
Females	2057
Total	4242

Among which were 48 pair of twins, and once *Tergemini*.

In *Ratisbon* died of the *Lutherans* 235 persons; amongst which were 117 children, 13 young men, and 11 young women.

The following list exhibits an account of several cities in *Saxony*.

	Christened	*Bastards.*	*Buried*	*Pair married.*	*Communicants.*
In *Dresden*	1578	99	1412	501	78875
Wittemberg	286	16	317	61	13536
Leipzick	861	68	953	303	
Torgau	136	9	148	54	7917
Freyberg	340	13	373	104	24098
Stollberg	88		112	27	7314
Pulsnitz	73		75	39	4381
Konigsbruck	67		95	16	3351
Elstra	63		45	11	3021
Bautzen	207	5	135	55	14520
Annaberg	146	19	112	37	8426
Chemnitz	173	4	166	38	10690
Oschatz	103	3	82	20	5169
Altenburg	141		248	57	12901
Eulenberg	96	5	86	41	6194
Pirna	138	2	133	43	9164
Marienberg	91		194	18	5903
Georgenstad	182		191	36	3580
Harta, near *Waldheim*	40	3	20	29	3291

A list for a whole century, *viz.* from 1617 to 1717 inclusive, giving an account of the numbers of people married, christened, buried and number of communicants in the electoral

electoral city of *Freyberg*, near the banks of the *Molde* in *Saxony*, Lat. 51. Long. 32° 43′ about 8 miles S. W. from *Dresden*.

Numb. Years	Pair married	Christened.	Bastards.	Buried	Communicants.
1617	89	539	4	430	10530
18	122	423	10	384	9631
19	124	495	13	371	9789
20	97	428	10	451	10093
21	106	430	6	507	9676
22	110	380	7	517	9536
23	100	357	8	589	9206
24	96	383	4	417	9509
25	98	375	7	454	10058
26	78	381	6	549	11642
27	108	372	7	331	11027
28	82	371	4	318	10745
29	87	412	19	536	11531
30	80	330	7	1114	11180
1631	135	353	4	284	10524
32	88	425	5	1343	11326
33	174	163	4	879	9131
34	170	375	1	175	7447
35	87	285	9	231	7323
36	61	299	11	181	7590
37	68	386	3	383	7723
38	53	262	9	151	7682
39	45	292	4	354	9609
40	65	268	6	173	7594
1641	61	226	2	183	7562
42	48	309	4	193	8128
43	60	383	7	340	7770
44	65	231	7	156	7699
45	62	196	5	163	7050
46	57	230	6	187	7263
47	59	201	2	136	7173
48	59	197	1	182	7698
49	44	222	5	201	7739
50	65	199	1	162	7639

Numb. Years.	Pair married	Christened.	Bastards.	Buried.	Communicants
1651	65	232	1	168	8148
52	50	190	2	202	7948
53	62	208	2	215	8342
54	62	234	3	211	8648
55	73	202	1	194	6351
56	60	237	5	151	8493
57	58	230		174	8730
58	61	198	4	184	8561
59	57	251	3	177	8431
60	66	230		240	8856
1661	47	225	3	217	8962
62	53	203	2	206	9423
63	47	206	2	247	9264
64	48	219	2	179	9279
65	61	213		207	9406
66	65	218	4	216	9979
67	79	223		306	9448
68	63	228	5	232	9588
69	55	232		204	9572
70	70	218	4	206	9564
1671	65	237	3	291	10080
72	64	207	2	286	9221
73	69	234	1	215	9912
74	55	228		204	9678
75	60	233	1	246	10093
76	58	226	1	277	9956
77	80	240	3	296	10462
78	70	256	4	302	10422
79	91	225	4	254	10356
80	63	292	4	267	11349

Numb. Year's.	Pair married	Christened.	Bastards.	Buried	Communicants.
1681	87	285	5	218	11043
82	69	282	7	253	10894
83	77	315	8	333	11054
84	78	334	6	314	10623
85	73	249	4	251	11070
86	73	282	5	243	11488
87	80	327	6	292	11542
88	88	308	11	234	11300
89	59	276	4	301	11393
90	72	290	5	310	11380
1691	72	269	6	289	11724
92	58	284	7	296	12679
93	72	316	10	253	11597
94	65	236	7	355	12533
95	57	239	6	339	13196
96	85	277	10	290	14900
97	76	216	4	180	15619
98	73	274	7	218	15677
99	64	264	5	258	16155
1700	74	251	7	310	18569
1701	81	283	10	188	19282
2	88	272	2	239	20383
3	95	271	6	241	20494
4	88	319	11	273	20837
5	72	295	12	357	20743
6	85	332	10	245	21813
7	82	310	18	378	21367
8	83	310	17	288	21838
9	71	302	10	305	21708
10	83	313	15	283	21699

Numb. Years.	Pair married	Christened.	Bastards.	Buried.	Communicants.
1711	81	327	8	311	22334
12	82	310	9	325	22837
13	87	308	9	340	23627
14	70	349	6	237	23470
15	82	323	8	283	23273
16	105	317	14	361	23371
17	79	373	13	325	23904
Total	7546	28851	582	30295	1211761

Here it is to be obſerv'd, that in the years 1625, 26, 27, 32, 33, the plague raged very much; as alſo that in 1630 there died meerly of the plague 764, and in 1680, 103: Beſides that, amongſt the chriſtened in 1713, a girl was baptiz'd, who was born and bred in *Turky*, but from Chriſtian parents.

But in general it may be obſerv'd, that this place was more populous before the war than it is at preſent; tho' in this century it cannot but very much recover and increaſe.

A liſt receiv'd from *Berlin*, giving an account of all that were born, married and buried in *Pruſſia* in four years.

	Year.	Born.	Pair married.	Buried.
In the kingdom of *Pruſſia*	1715	19606	4571	12000
	16	20669	4530	12155
	17	21443	4743	12301
	18	20994	4287	11047
	Sum	82712	18331	47503

The following liſt contains the number of all that were born, married, and buried in the reſt of the King of *Pruſſia*'s dominions in *Germany*, &c. for four yerrs.

	Year.	Born.	Pair married.	Buried.
In the electorate of *Brandenburg*.	1715	14820	4368	11663
	16	15758	4995	13149
	17	16636	4539	10805
	18	17319	4657	12891
	Sum	64533	18559	48508

	Year.	Born	Pair married	Buried.
In the *Newmarck.*	1715	5487	1683	4757
	16	4973	1010	4502
	17	7052	1850	4673
	18	6684	1882	5069
	Sum	24196	7425	19001
In the dutchy of *Magdeburg.*	1715	7185	2064	4982
	16	7225	2186	5602
	17	8746	2306	6291
	18	8325	1934	5744
	Sum	31481	8490	22619
The dutchy of *Cleve.*	1715	6772	1964	5066
	16	7313	2001	6221
	17	7582	2051	6055
	18	7542	1891	5865
	Sum	29209	7907	23207
The dutchy of *Pomerania.*	1715	7243	2190	5373
	16	7386	2407	5557
	17	8196	2108	5020
	18	7896	2138	6231
	Sum	30721	8843	22181
The principality of *Halberstad.*	1715	2371	710	1834
	16	2567	759	2542
	17	2759	764	1961
	18	2527	715	2022
	Sum	10224	2948	8359
The county of *Hohensteim.*	1715	528	164	406
	16	639	155	444
	17	644	148	449
	18	612	158	416
	Sum	2423	625	1715

The

	Year.	Born.	Pair married.	Buried.
The principality of *Minden*.	1715	1711	579	1618
	16	1788	564	1829
	17	1991	542	1419
	18	1832	552	1486
	Sum	7322	2237	6352
The city of *Meurs*.	1715	406	151	386
	16	439	161	331
	17	454	166	281
	18	470	170	368
	Sum	1769	648	1366
In *Geldern*.	1715	—	—	—
	16	—	—	
	17	1990	497	1066
	18	2053	489	1064
	Sum	4043	986	2130
The county of *Ravensberg*	1715	2188	679	1987
	16	2410	716	2349
	17	2312	672	1817
	18	2344	641	1951
	Sum	9254	2708	8104
The county of *Tecklenberg*.	1715	342	153	513
	16	553	163	543
	17	505	186	379
	18	507	135	387
	Sum	1907	637	1813
The county of *Lingen*.	1715	628	213	484
	16	699	154	638
	17	697	248	449
	18	706	221	840
	Sum	2730	836	2411

In

	Year.	Born.	Pair married.	Buried.
In *Lauenburg* and *Butow*.	1715	585	144	362
	16	587	169	420
	17	586	118	366
	18	554	153	321
	Sum	2312	584	1469
In the *French* colonies.	1715	—	—	—
	16	—	—	—
	17	607	181	564
	18	603	136	639
	Sum	1210	317	1203

The sum total of all that were born in four years. } 306046
of all that were married 81881 pair.
of all that were buried 217941
More born than buried in number 88105

Here it is alſo to be obſerv'd, that in the year 1718, there died 84 perſons upwards of 91 years of age, and 32 upwards of 100, and one in the 116th year of his age: Beſides, in that very year there are reckoned 2088 baſtards.

A particular liſt, (receiv'd from the chancery of *Konings-berg*) of all that died in the year 1718, in the ſeveral borough towns in the kingdom of *Pruſſia*.

In the borough of	*Died*	*In the borough of*	*Died*
Brandenburg	581	Neudenberg *and* Saldau	330
Schacken	346	Lyck	141
Fiſhauſen	312	Oletzko	188
Taxium	456	Angerburg	182
Balga	392	Rhein	107
Preuſs Eylau	319	Orſelsburg	220
Bartenſtein	167	Johannisburg	141
Barthen	185	Lotzen	137
Oſterode *and* Hohenſtein	277	Neuhauſen	148
Scheſten	159	Labiau	249

In

In the borough of	Died	In the borough of	Died
Rastenburg	275	Mummel	556
Preuss Holland	264	Schonberg	126
Marungen *and* Liebstadt	206	Gerdauen *and* Nordenburg	269
Marienwerder *and* Riesenburg	393	Gilgenburg	109
Preuss Marck	312	Teutsch Eylau	41
Tilsit	529	Neuhaff	15
Ragnit	381	*In the three cities of* Koningsberg	1756
Insterburg, Georgenburg, Salau	808	Sum	11077

In all these above-mentioned places were christened 20994
Pair married 4287

	Died.	Christened.	Married.
In *Dantzick* in the year 1718.	1579	1470	446 pair
In *Epperies*	123	173	

In the royal hospital at *Lisbon* out of 1251 foundlings, maintained by the King, there died 469, remained 782.

Experiments concerning the Degrees of Heat of boiling Liquors; *by M.* Fahrenheit. Phil. Transf. N° 381. p. 1. *Translated from the* Latin.

M. *Fahrenheit*, finding in the History of the Royal Academy of Sciences, that the celebrated M. *Amontons* had, by means of a thermometer of his own invention, discover'd, that water boils with a fixt degree of heat, was very desirous of making such another thermometer, in order to view with his own eyes this beautiful phænomenon of nature, and be convinced of the truth of the experiment.

M. *Fahrenheit* recollected what that sagacious enquirer into nature, had writ about the manner of rectifying barometers; for, M. *Amontons* had observ'd, that the height of the column of mercury is somewhat, nay, sensibly affected by the different temperament of the mercury: From this M. *Fahrenheit* imagined, that perhaps, a thermometer might be made of mercury, whose structure would not be so difficult, and yet by it he might discover the experiment he so much desir'd.

Having

Having made ſuch a thermometer (tho' ſtill imperfect in ſeveral reſpects) the event anſwer'd his expectation to his no ſmall ſatisfaction.

The iſſue of the experiments is exhibited in the following table, the firſt column ſhews the ſeveral liquors that were made uſe of, the ſecond their reſpective ſpecific gravities; the third the degree of heat each liquor acquir'd by boiling.

Liquors.	The ſpecific gravity of liquors heated to 48 degrees.	The degrees of heat acquir'd by boiling.
Spirits of wine	8260	176
Rain water	10000	212
Spirit of nitre	12935	242
Lixivium of pot-aſh	15634	240
Oil of vitriol	18775	546

M. *Fahrenheit* thought it neceſſary to add the ſpecific gravity of each liquor, that if the experiments, either already made, or hereafter to be made by others, ſhould happen to differ from the above-mentioned, it might be known whether the difference were owing to the variation of the ſpecific gravity, or to other cauſes: Moreover, the experiments were made at different times, and hence likewiſe the liquors were affected with various degrees of heat; but becauſe their gravity is variouſly and unequally diſturbed, he reduced it to 48 degrees, which in his thermometer is the mean between the moſt intenſe artificial cold, made by the commixture of water, ice, ſal-armoniac, or even ſea-ſalt, and the degree of heat of the blood of a healthful perſon.

It is true, volatile oils begin to boil with any degree of heat; but their heat continually increaſes by boiling; the reaſon of which may, probably, be this, *viz.* that the more volatile particles fly off, while the reſinous ones, that have a greater attraction, remain behind.

But fix'd oils require ſo great a degree of heat, that the mercury in the thermometer begins to boil at the ſame time with them, and hence their degree of heat can ſcarce be found with certainty in the manner above-mentioned.

Excepting ſpirits of wine and water, probably, the degrees of heat in the other liquors, above-mentioned, may likewiſe vary; eſpecially, if they be taken in larger quantities, and boil ſome time.

Obſer-

Observations made in 1723, *on the* Barometer, Thermometer *and* Hygrometer, *&c. by M.* Nicolaus Cruquius. Phil. Transf. N° 381. p. 4. *Translated from the* Latin.

IN 1723 M. *Cruquius* made the following accurate observations at *Leyden*, *Delft* and *Rheinburg*, on the mean height of the barometer and thermometer, as also on the mean variation of the hygrometer; the quantity of rain, dew, snow, hail, as also the quantity of water exhaled; and the height of the water in a well, out of which none had been drawn for a whole year; and on the monthly variation of a pocket-watch.

	Barometer	Thermometer	Hygrometer	
	Here M. *Cruquius* supposes the weight of the atmosphere incumbent on the plane of a *Rheinland* square foot; the weight of which atmosphere is equal to 1947 *Amsterdam* pounds, when the height of the mercury in the barometer is 27 inches 7 lines; and 2094 *Amsterdam* pounds, when the height of the mercury is 29 inches 8 lines. This was observed to be the greatest and least degree of height of the mercury in the barometer for several years.	Here he determined the heat of the atmosphere in the place of observation from the rarefaction of the air, so that the greatest degree of cold was observed, at 1000 degrees, pure water froze at 1070 degrees, and boiled at 1510.	Here he measur'd the quantity of water in the atmosphere in the place of observation according to the increase or decrease of the weight of a sponge hung from a balance after first moistening it in a solution of sal armoniac.	The quantity of water exhaled at *Rheinburg* into the open air in each month throughout the year 1723.
	pds.	dgr.	weight	lin.
January	2051	1076	81	7
February	46	85	80	14
March	35	102	80	33
April	46	109	66	36
May	57	126	57	58
June	53	140	57	57
July	2044	1129	58	37
August	46	141	60	39
September	54	132	61	24
October	55	121	71	15
November	53	104	77	15
December	35	96	79	12
Sum	575	1361	821	347
12				
Mean weight	2048	1113	68	28 inches 11 lines the whole year.

The

The height of rain, dew, snow and hail; particular care being taken that not the least quantity was lost by exhalation or any other way.		The height of the water in a well, measur'd from its brink to the surface of the water at the end of each month; from which no water was drawn all the time of the observation.	The variation of an accurate pocket-watch; shewing how many minutes it goes too fast or too slow every month; + denoting the increase and ÷ the decrease of its motion.
At *Delft* *lin. decim.*	At *Rheinburg* *lin. decim.*	feet inch.	minutes.
— 17.9	— 21.2	— 5 : 4	— + 151 *Jan.*
— 25.1	— 23.8	— 4 : 11	— + 21 *Febr.*
— 18.8	— 28.	— 5 : 7	— ÷ 168 *Mar.*
— 5.5	— 7.5	— 6 : 10	— ÷ 120 *April*
— 4.2	— 2.7	— 8 : 1	— ÷ 123 *May*
— 3.2	— 4.8	— 9 : 3	— ÷ 130 *June*
— 38.6	— 28	— 9 : 7	— ÷ 90 *July*
— 41.9	— 40.2	— 9 : 7	— + 133 *Aug.*
— 15.1	— 14.8	— 9 : 9	— + 24 *Sept.*
— 8.2	— 11.3	— 9 : 8	— + 19 *Octob.*
— 30.7	— 29.7	— 9 : 3	— + 266 *Nov.*
— 30.6	— 40.	— 8 : 2	— + 252 *Dec.*
— 239.8	— 25.2	96 : —	+ 866, & ÷ 631 or + 235
At *Delft* 20 inches and at *Rheinburg* 21 inches for the whole year.		mean height 8 foot.	20′ too fast every month.

Obser-

Observations made for a number of years.

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	mean	ſum	year
	lb.					BAROMETER							12	lb.	
1720	2034	34	38	36	38	42	45	36	50	40	38	23	454	2038	1720
1721	52	42	36	29	29	43	51	46	43	42	32	29	474	39	1721
1722	76	41	32	41	42	45	36	39	50	59	37	30	528	44	1722
1723	51	46	35	46	57	53	44	46	54	55	53	35	575	48	1723
	213	163	141	152	166	183	176	167	197	196	160	117		169	
4	2053	41	35	38	41	46	44	42	49	49	40	29		2042	
	deg.					THERMOMETER								dgr.	
1720	1087	89	86	102	126	126	145	138	121	107	91	91	1309	109	1720
1721	90	74	75	112	116	137	136	142	133	110	100	84	1309	109	1721
1722	79	90	97	109	122	134	139	140	135	117	101	90	1353	113	1722
1723	76	85	102	109	126	140	129	141	132	121	104	96	1361	113	1723
	332	338	360	432	490	537	549	561	521	455	396	361		444	
4	83	85	90	108	122	134	137	140	130	114	99	90		111	

Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	mean	ſt my	
	weight			HYGROMETER									weight		
1721	89	82	73	80	69	64	63	68	76	76	89	88	917	76	1721
1722	83	85	76	63	62	62	63	68	70	76	81	88	882	74	1722
1723	81	80	80	60	57	57	58	60	61	71	77	79	821	69	1723
	258	247	229	203	188	183	184	196	207	223	247	255		219	
3	86	82	76	68	63	61	61	65	69	74	82	85		73	
	lines			Rain, &c. at Delft.										inch : lin.	
1715	11	20	36	8	15	18	95	62	36	37	47	15	400	33 : 4	1715
1716	19	20	14	7	17	4	48	19	55	57	21	32	313	26 : 1	1716
1717	31	15	29	29	31	28	29	28	24	32	29	28	333	27 : 9	1717
1718	21	18	6	30	17	17	35	27	14	46	21	25	277	23 : 1	1718
1719	33	20	10	11	17	4	12	34	23	25	24	22	235	19 : 7	1719
1720	36	24	21	21	15	20	23	55	47	56	25	20	363	30 : 3	1720
1721	20	31	27	59	30	34	15	41	27	57	30	48	419	44 : 11	1721
1722	2	20	25	23	15	22	49	53	25	7	21	53	315	26 : 3	1722
1723	18	25	19	5	4	3	39	42	15	8	31	31	240	20 :	1723
	191	193	187	193	161	150	345	361	266	325	249	274		241 : 3	
9	21	21	21	21	18	17	38	40	30	36	28	30		26 : 10	
	lines.			Rain, &c. at Rheinburg.											
1720	35	16	13	24	29	14	20	48	54	50	39	25	367	30 : 7	1720
1721	21	29	28	59	35	28	14	47	27	51	32	46	417	34 : 9	1721
1722	3	20	27	29	28	27	60	64	26	10	21	61	376	31 : 4	1722
1723	21	24	28	7	3	5	28	40	15	11	30	40	252	21 :	1723
	80	89	96	119	95	74	122	199	122	122	122	172		117 : 8	
4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		29 : 5	

Years	*Jan.*	*Feb.*	*March*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*		feet inch.	
	lines.			Water-exhaled at *Rheinburg*.											
1720	10	10	10	25	44	55	59	56	50	30	10	12	371	30 : 11	1720
1721	8	9	15	54	47	60	48	42	27	20	22	10	362	30 : 2	1721
1722	5	7	20	28	54	54	65	59	34	14	13	13	366	30 : 6	1722
1723	7	14	33	36	58	57	37	39	24	15	15	12	347	28 : 11	1723
4	30	40	78	143	203	226	209	196	135	79	60	47		120 : 6	
	8	10	20	36	51	56	52	49	34	20	15	12		30 : 2	
	feet inch.			The height of the water in a well.										feet inch	
1720	6 :	6 : 3	6 : 5	6 : 7	6 : 6	8 :	8 : 11	9 :	8 : 3	6 : 11	5 : 8	5 : 5	84 :	7 :	1720
1721	5 : 6	5 : 6	4 : 6	4 : 2	5 : 7	7 :	8 : 1	7 : 9	7 : 7	6 : 3	4 : 6	4 : 4	70 :	95 : 11	1721
1722	6 :	5 : 7	5 : 11	6 : 1	6 : 10	7 : 11	8 :	7 : 10	7 : 4	7 : 9	7 : 3	4 : 6	81 :	6 : 9	1722
1723	5 : 4	4 : 11	5 : 7	6 : 10	8 : 1	9 : 3	9 : 7	9 : 7	9 : 9	9 : 8	9 : 3	8 : 2	96 :	8 :	1723
4	22 : 10	22 : 3	22 : 5	23 : 8	27 :	32 : 2	34 : 7	34 : 2	32 : 11	30 : 7	26 : 8	22 : 5		27 : 8	
	5 : 8	5 : 7	5 : 7	5 : 11	6 : 9	8 :	8 : 8	8 : 6	8 : 3	7 : 8	6 : 8	5 : 7		6 : 11	
	min.			The variation of a pocket watch.											
1722	÷ 34	÷ 9	÷ 7	+ 5	÷ 38	÷ 55	÷ 89	÷ 78	÷ 84	÷ 8	+ 151	+ 246	+ & ÷	402	1722
1723	+ 131	+ 1	÷ 187	÷ 139	÷ 142	÷ 149	÷ 109	+ 113	+ 4	÷ 1	+ 246	+ 232	+ & ÷	727	1723
2	+ 97	÷ 8	÷ 194	÷ 134	÷ 180	÷ 204	÷ 198	+ 35	÷ 80	÷ 9	+ 397	+ 478	+ & ÷	1129	
	+ 48	÷ 4	÷ 79	÷ 67	÷ 90	÷ 102	÷ 99	+ 17	÷ 40	÷ 4	+ 199	+ 239	+ & ÷	564	
	Jan.	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*			

A Dropsy

A Dropsy *in the left* Ovarium *of a Woman*, 58 *Years of Age, cured by a large Incision made in the side of the* Abdomen; *by Dr.* Robert Houston. Phil. Transf. N° 381. p. 8.

Margaret Millar in her last lying-in, at 45 years of age, having the burden violently pulled away, was so sensibly affected with a pain, which then seised her left side, between the navel and groin, that ever after she had scarce been free from it; but had it more or less for 13 years together; for two years before the Dr. had seen her, she was extremely uneasy, her belly grew very large, and a difficulty of breathing continually increased upon her; insomuch, that for the last six months, she had scarce breathed at all but with the utmost difficulty; and in all that time, having quite lost her appetite, she scarce eat so much as would nourish a sucking child; and for three months together she had been obliged to lie constantly on her back, not daring to move to one side or other.

This tumour was grown to so monstrous a bulk, that it engross'd the whole left side, from the navel to the *pubes*, and stretched the abdominal muscles to so unequal a degree, that the Dr. never observed the like in the whole course of his practice. It drew towards a point: Her back, by lying so long in that posture, was excoriated, which, with want of rest and appetite, had greatly wasted her.

The Dr. encouraged by the unexpected resolution and importunity of the patient, with an imposthume lancet, laid open about an inch; but finding nothing issue, he enlarged it two inches; and even then nothing came out but but a little thin yellowish *serum*: So he ventured to lay it open about two inches more. He was not a little surprised, after so large an aperture, to find only a glutinous substance stop up this orifice: All the difficulty was to remove it: He tried his probe, and his fingers, but all in vain; it was so slippery, that it eluded every touch, and the strongest hold he could take of it.

He took a strong fir-splinter, about the end of which he wrapped some loose lint and thrust it into the wound, and by turning and winding it, extracted upwards of two yards in length of a substance, thicker than any jelly, or rather like glue, that is fresh made and hang out to dry; its breadth was upwards of 10 inches: This was followed by nine full quarts of such matter, as is observed in steatomatous and atheromatous tumours, with several *hydatides* of various sizes, containing a yellowish *serum*, the least of them bigger than an orange,

with

with ſeveral large pieces of membranes, which ſeemed to be parts of the diſtended *ovarium:* Then he ſqueeſed out all he could, and ſtitched up the wound in three places, almoſt equidiſtant. He was obliged to make uſe of *Lucatellus*'s balſam; with which he covered a pledget the whole length of the wound, and over that he laid ſeveral compreſſes, dipped in warm *French* brandy; and becauſe he judged, that the parts might have loſt their ſpring, by ſo conſiderable and ſo long a diſſention, he dipped in the ſame brandy a napkin, and applied it four fold over all the dreſſings, and with a couple of ſtrong towels, which were alſo dipped in the brandy, he ſwathed her round the body, and then gave her about four ounces of the following mixture.

℞ *Aq. Menthæ lb ſs. Aq. Cinnamoni fort. lb* 1 *ſs. Syr. Diacodii* ℥vi. *M.*

The cinnamon water was drawn off from canary and the beſt cinnamon, being the fineſt and moſt fragant he ever taſted; of this mixture he ordered her two or three ſpoonfuls four times a day.

Next morning he found the patient in a breathing ſwear; and ſhe informed him, that ſhe had not ſlept ſo much, nor found herſelf ſo well refreſhed, at any time for three months before: He carefully attended her once every day, and as conſtantly dreſſed her wound in the ſame manner as above, for about eight days together: He kept in the lower part of the wound a ſmall tent, which diſcharged ſome ſeroſities at every dreſſing for four or five days.

Her chief food was ſtrong broth, made of an old cock, in each porringer of which was put one ſpoonful of the abovementioned cinnamon water; this was repeated four times a day, and gave her new life and ſpirits.

She mended apace to the admiration of every body, and lived in perfect health from that time, *viz. Auguſt* 1701 till 1714, when ſhe died of 10 days illneſs.

That this tumour, or rather dropſy of the *ovarium*, was owing to the midwife's raſhneſs in pulling away the *placenta*, not knowing how to ſeparate it ſkillfully from the *uterus*, ſeems plain from what the patient herſelf told, and what afterwards happened.

The *placenta* adhering ſo faſt to the *uterus* required more art to bring it away than the midwife was miſtreſs of, which probably induced her to uſe violence; by which ſhe forced down the *fundus uteri*, and ſo overſtrained the ligaments and all that is appended to them; eſpecially, the *ligamentum latum* of the left

left side and its *ovarium*, which may be reasonably supposed to have been hurt with the rest in the relaxation: Hence the elasticity of these parts was not only impaired, but the small lymphatics ruptured: So that the extravasated *lympha*, rushing out, thickened, and not being able to recirculate, dilated the injured *ovarium*, and thus increased the tumour; and the parts being already excessively distended, and no longer able to resist the new influx of fresh secretions, were likewise ruptured, and by degrees augmented to that enormous bulk.

Cyprianus, in his letter to Sir *Thomas Millington*, gives several instances of the mischiefs committed by midwives and other ignorant persons: When a *placenta* adheres, then says he they tear all before them; by pulling rudely, they force down the *fundus uteri*; consequently, do violence to the *ovaria* and other parts: Whence follow inflammations, *&c.*

Forestus l. 28. *obs.* 80. says that an over forward midwife pulled out the *placenta* too hastily, and that the woman immediately fell into a swoon, and died directly.

Frederick Ruysch in his *obs. Anatom.* 97. attributes the cause of the close adhesion of the *placenta*, to a central insertion of the navel-string into it, in which case it is hard to separate: A train of dismal accidents attend those, who, not knowing how to do it, venture to pull it away by force.

Dionis and *La Motte* have made the same remark; by the latter we are told of innumerable accidents, which he has known happen to women in hard labour, by the rashness and ignorance of bold pretenders, who, without rule, precepts, practice, or any suitable instructions, venture to practice midwifry.

The abovementioned *Ruysch* has a remarkable instance of the effects of violence in hard labour; the case is curious. vide *Obs. Anat.* 63.

Manget, in his *Theatrum Anat.* tells us, that in all hard labours, where the *placenta* sticks, the ligaments suffer more or less by a rash and ignorant way of pulling: Examples of which see in *Platerus's Obs. lib.* 3. *Bartholin. Cent.* 2. *Hist.* 91. *Cent* 5. *Hist.* 19. *Marchett. Anat. cap.* 7. and others, of which *Blasius* has given us an ample list in his commentaries on *Veslingius*.

He likewise tells us, that the *ovaria* grow to a surprising bulk and contain such a vast quantity of liquor, that they become dropsical; whereof *Skenckius* in his observations has recorded many examples, as has *Riolan* and several others.

Mun-

Munnics, in his *Bibliothec. Anat.* gives us the history of a large dropsy in the right *ovarium*.

Mortgagni has told us, that he has often observ'd large vesicles in the *ovaria*, and in morbid bodies, sometimes full of purulent matter.

Sylvius. Ex lymphaticis læsis tam virilium quam mulierium testiculorum, &c. *hydrops particularis excitari potest.*

Vesingius cap. 7. has often observ'd large tumours, from obstructions in the *ovaria* of women.

Vesalius found in the right *ovarium* 9 or 10 glandules, like goose eggs, with matter not unlike the whites of eggs, or rather thicker.

Gul. Ballon. paradig. 6. found an imposthume of the nature of a *steatoma*, near the *collum uteri*; the woman had a large tumour on her side for 16 years, and had only one *ovarium*, which was shewn as an extradiordinary thing.

At *Westminster*, Dr. *Houston* opened the body of a gentlewoman, about 60 years of age, whose left *ovarium*, of a large size, weigh'd six pounds, to which there adher'd several *hydatides* like pullets eggs.

Hildan mentions an hydropic tumour of the right *ovarium*, of a prodigious bulk, stuff'd with hair. vide *Gul. Fab. Hild. Cent.* 5. *Obs. Horst, Tom.* 2. *lib.* 4. *Obs.* 53.

Ruysch. Obs. Anat. 17. has a passage to the following purpose, 'a dropsy, says he, of the *ovaria* is an affection or disease, well enough known to others; but I am afraid not so well considered: Authors have agreed to call it *hydatis*, with which name I shall rest satisfied, provided we agree about the subject of the distemper. I observe this most frequently, if not always to be the *ova*; but so changed and sometimes swell'd to such a bulk, that I have often seen them bigger than a child's head'.

Drelincourt has given us a very full and exact account of dropsy of the left *ovarium* in a Lady of 35 years of age: The tumour, of an enormous bulk, continued three years. The body of the *ovarium*, with all contain'd therein, weigh'd 80 pounds. It was nothing but a number of little globules, cluster'd together, that differ'd in their roundness, form, colour and consistence, proceeding from little seminary vesicles in a cluster: Some contain'd water exceeding clear and liquid; others a yellowish thin *serum*; others a glutinous matter; some were as big as pullets eggs; others bigger than one's fist.

These few, out of the many instances could be produced from authors of undoubted reputation, suffice to prove, th the *ovaria*, as well as the *tubæ fallopianæ*, ligaments and *uteri* itself, are not free from dropsies, *&c.* and that they a owing to obstructions, often occasioned by rude and violen dealing with women in hard labours, which generally brin on a train of dismal symptoms, that sooner or later, accordin to the strength of the patient, after a miserable, painful an languishing life, end in death.

The manifest success in the above-mentioned uncommo case, may be of use, and may serve to shew, that we oug not to despair so soon, in distempers that are seemingly mo dangerous.

The Preparation of the Prussian Blue. Phil. Trans. N° 38 p. 15. *Translated from the* Latin.

DR. *Woodward* communicated to the *Royal Society* th following preparation of the *Prussian* blue, as it w sent him from *Germany*.

Take 4 ounces of crude tartar and crude nitre, dried, pu verise them very fine, and then mix them; afterwards l them decrepitate by applying a live coal; and you'll hav 4 ounces of extemporaneous salt of tartar. While this salt hot, pulverise it very fine, and add to it 4 ounces of ox blood, well dried and reduced to a fine powder. Aft mixing them well together, put them into a crucible, that it be $\frac{2}{3}$ full; then covering the crucible, put it into fire, and surround it with live coals, that it may heat gradu ally, and that the matter may take fire and glow without to quick an accension: Let the matter be kept in this degree heat, till the flame and accension remit; at length encrea the fire, till the matter be quite glowing hot, and little or n flame arise from the crucible; take the crucible off the fir and putting the matter into a mortar, pound it a little, an having at hand 4 pounds *avoird.* of boiling hot rain-wate put the matter hot into it, and boil it for half an hour; aft which strain it thro' a linnen cloth, and pouring some mo water upon the remaining black matter, put it again up the fire, boil it and strain it; and this must be repeated often, till the matter be well edulcorated, and the wat remain insipid: Express well all the liquor remaining in th

Line

linen cloth and matter; and when you have collected the whole together, set it again upon the fire, and evaporate it to 4 pounds, and keep it by for farther use, marking it N° 1.

Take 1 ounce of *English* vitriol gently calcin'd to a white colour, dissolve it in 6 ounces of rain-water; filtre it thro' a paper, and mark it N° 2.

Lastly, take 8 ounces of crude allum; dissolve it in 4 pounds of boiling hot water, till all the alum be consum'd, after which add the solution of vitriol N° 2, which put hot from the fire into a pretty large and wide pot, and combine it with the *lixivium* N° 1, well heated apart: And immediately a great ebullition will arise, and yield the colour of borax; during the ebullition, pour it alternately out of one vessel into another; and when it ceases, let it stand; then strain it thro' a linen cloth, that the liquor may pass thro', and the colour remain in the cloth; and when no more liquor drops from it, remove it from the cloth with a wooden spatula into another smaller pot, after which pour on it 2 or 3 ounces of spirit of common salt, and there will immediately appear a very beautiful blue colour; mix this very well, and let it stand all night; afterwards put to it a large quantity of rain water, stir it about with a spatula, and when the matter has subsided, decant the water, and pour on fresh, still repeating this, till it is adulcorated, and the water that runs at length from it be insipid: Put this deep coloured blue precipitate upon a linen cloth spread, that the water may run off, and let the colour be gradually dried with a gentle heat for use.

N. B. The calcination is of considerable service in this preparation: For, a bright azure and a deep blue arise from a gentle, moderate, or strong calcination of the dried blood and salt of tartar; and hence arises the diversity of colour: The boiling hot *lixivia* are to be directly mix'd together.

Observations and Experiments on the foregoing Method of Preparation; by Mr. John Brown. Phil. Transf. N° 381 p. 17.

DR. *Woodward* having in the preceeding *Transaction* communicated to the *Royal Society* a process for making the *Prussian* blue; Mr. *Brown* was willing to go thro' it exactly, according to the proportions there prescrib'd; and he observ'd, that by a calcination of 4 ounces of blood dried with 4 ounces of the salt of tartar, in two hours that part of the operation was over, and a black spongy substance remain'd

remain'd in the crucible, weighing 4 ounces *avoirdupois*: A ſolution of which being made in boiling water, and afterwards filtred, the remainder, when dried, weigh'd 9 drachms *averdupois*.

The loſs in the ſolution and filtration of the vitriol and alum, is not worth taking notice of, they having both been very clean, before they were diſſolv'd: The mixtures being made, as preſcrib'd, with the addition of the ſpirit of ſalt, the reſult was a very fine blue, which when well edulcorated by frequent waſhings, and after that thoroughly dried, weigh'd 1 ounce or a little more, and entirely anſwer'd the character the author gave of it.

Among the ſeveral experiments, made with theſe liquors, to wit, the *livivium* with blood, the ſolution of vitriol, the ſolution of alum, and the ſpirit of ſalt; tho' they always produced a blue, yet that blue differ'd in degrees of colour, according to the varied proportions of the vitriol and alum; and the colours produced from theſe ſeveral proportions were each of them improv'd by the addition of the ſpirit of ſalt.

Mr. *Brown* only mentions two out of the ſeveral experiments he tried, in one of which the alum was entirely left out, and a pale blue produced; in the other the proportions of vitriol and alum were equal, and a very deep blue was produced.

Theſe differences in colour, ariſing from the ſeveral proportions of the vitriol and alum, are only mentioned to confirm the truth of the author's preſcript, as being the moſt exact and beſt proportioned to produce the fineſt colour, of any he tried. The only misfortune he takes notice of, attending his preſcript, is what may happen in the calcination.

It would be curious to know what gave the firſt hint for the production of ſo fine a colour, from a combination of ſuch materials; eſpecially, when we come to conſider, that the blood has the greateſt and principal ſhare in this ſurpriſing change: Mr. *Brown* doubts not, but blood, or fleſh of any kind, would produce the ſame effects, but he had reaſon to think the latter would not produce ſo beautiful a colour as the former. He on purpoſe dried ſome beef, freed from its ſkin and ſalt, and purſu'd the ſame proceſs, as with the blood; but there was a ſenſible difference obſervable during the calcination, and a very manifeſt one in the beauty of the two colours.

To

To prove the ſhare the blood has in this change, the following experiments (ſome of which Mr. *Brown* made before the *Royal Society*) may be concluſive.

The ſolution of alum, mix'd with that of the vitriol, produced no alteration of colour; if to theſe you add the ſpirit of ſalt, the appearance is the ſame: But if to the whole you put the *lixivium* with blood, there precipitates a blue.

If you ſubſtitute, inſtead of the *lixivium* with blood, a *lixivium* made with the ſame ſalt of tartar only, which then becomes an oil of tartar; and after the mixture of the ſolution of alum, with that of the vitriol, you pour on this oil of tartar, there follows, it is true, a precipitation, but which is of no colour; and if you add the ſpirit of ſalt, it ſo ſtrongly attracts what is precipitated, as to render the muddy mixture perfectly clear.

The very ſame effect will follow, if any volatile alkalious ſpirit be made uſe of as a precipitant; or any volatile ſalts diſſolv'd in water; nor can the blood itſelf be ſuppos'd to communicate this change from any ſuch properties, the heat of fire it undergoes in the calcination being ſufficient to throw them off.

In the calcination of the dried blood and ſalt of tartar, it was obſerv'd, that there was a loſs of juſt the half. It is difficult to determine exactly what quantity of either was loſt by this calcination; but it will eaſily be granted, that there was loſt a far larger quantity of the blood, than of the ſalt of tartar; and that is obvious from an experiment, by which, when the ſalt of tartar was calcin'd by itſelf, with the ſame degree of heat, it loſt leſs than an $\frac{1}{8}$ part; whereas, when the dried blood was calcin'd by itſelf, it loſt more than $\frac{6}{8}$.

The blood, in calcination with the ſalt of tartar, communicates its tinging quality to the ſalt, or that quality is extracted from it by the ſalt, and paſſes with it in its ſolution in the boiling water.

To prove this, ſome dried blood was calcin'd by itſelf, and a ſtrong decoction was made of it in water, and afterwards filtred: This, when mix'd with the former ſolutions, produced little or no alteration; but on the addition of the ſpirits of ſalt, changed to an amber colour, without any precipitation.

When this liquor was mix'd with the oil of tartar, and pour'd on the former ſolutions, it caus'd a precipitation; but no colour, and the ſpirit of ſalt, as in the other experiment,

ment, made the liquor clear again, but left this also of an amber colour.

The change of colour is not effected in any of the materials, excepting in that of the solution of vitriol; so that the alum seems only to be of use in fixing the colour, as it is often us'd by the dyers for that purpose, and the spirit of salt gives it a deeper dye: For, if the *lixivium* with blood be poured on the solution of alum alone, there will fall a sediment, somewhat on the purple; to which if you add the spirit of salt, it changes the colour, and the sediment becomes brown.

So, much the same changes will be produced, if you pour the spirit of salt to the *lixivium*; but not the least appearance of a blue: Whereas, as is mentioned above, when the *lixivinm* is pour'd to the solution of vitriol, there immediately follows the blue, which is still heightened by the addition of the spirit of salt.

It will not be improper to observe, that as the author orders all the liquors, excepting the spirit of salt, to be boiling hot, when mix'd; so it is certain, the colour is thereby more immediately produced, and looks more beautiful: But most of the experiments here mentioned were made with the liquors cold, and the colours came to their beauty with a little washing. In one of the experiments with the liquors cold, after the *lixivium* with blood had precipitated the blue in the mixture of alum and vitriol, by pouring in a little more of the *lixivium*, the blue all disappear'd, and a dirty muddy colour was left: But the addition of the spirit of salt soon discharged that, and the blue return'd.

In calcining the beef and salt of tartar, Mr. *Brown* found the matter left in the crucible, to weigh just half the whole mixture, as in that with the blood: But after the boiling it in water, the remainder in the filtre, when dried, was very near a third less in proportion than the other. Whence it may be reasonably inferr'd, that the salt of tartar holds a larger share of the beef in the one operation, than of the blood in the other.

Having in the former part of this account of the *Prussian* blue prov'd, by the experiments there mentioned, that the solution of vitriol was the only subject among those ingredients, that the *lixivium* of blood produced this change of colour in; and afterwards considering, that the vitriol, made use of in this preparation, is no other than iron, dissolv'd by a liquor, running from the *pyrites*, when expos'd to the weather

weather, and which is afterwards boil'd up and ſhoots into cryſtals: It ſeem'd naturally to follow, that this metal is the ſubject, on which the *lixivium* of blood produces the change; and this thought gave occaſion to the following experiments on metalline bodies, in order to obſerve, whether the ſame change of colour could be produced in any of them.

To a ſolution of ſilver in *aqua fortis* was pour'd the *lixivium* of blood, which caus'd a *coagulum* of a pure fleſh colour. The *lixivium*, made with fleſh, produced a whitiſh *coagulum*, and the oil of tartar (which was continued to be us'd by way of compariſon with the other *lixivia*) a much whiter *coagulum*. By the addition of the ſpirit of ſalt to each of theſe, the bloom of the fleſh colour was taken off in the firſt, but it ſuffer'd no other change. In the ſecond, the *coagulum* was ſomewhat tinged blue; and in the third the white was manifeſtly improv'd. The blueiſh tinge in the ſecond of theſe experiments cannot entirely be aſſign'd, as the effect of the *lixivium* with fleſh; becauſe ſilver, when thus diſſolv'd, whether precipitated with ſalt water, or oil of tartar, will, after it has ſtood ſometime, contract a blueiſh tinge, and this from an alloy of copper, from which it is not entirely freed.

The ſame liquors were made uſe of to precipitate the mercury in the *Mercurius ſublimat. corroſ.* diſſolv'd in water; the conſequence was, that the *lixivium* with blood produced a pure yellow; the *lixivinm* with fleſh an orange colour; and the oil of tartar a dingy red: The addition of the ſpirit of ſalt to theſe made ſome very odd alterations; for, the firſt changed its yellow colour for an orange; the ſecond its orange for a blue; and the third became quite clear again, without any colour. The blue colour, in the mixture of the *livivium* with fleſh and ſolution of the ſublimate, may be accounted for from the vitriol in the compoſition of the ſublimate: But it will not be ſo eaſy to give a reaſon, why the ſame colour ſhould not have been produced from the *lixivium* with blood, and the ſame ſolution.

Copper, diſſolv'd in *aqua fortis*, tinges the water of a green colour, and if to this you pour the two *lixivia* of blood and fleſh, the *coagula* are much alike, *viz.* a white tinged with green: But when you add the ſpirit of ſalt, they both change and become of a colour, not unlike the copper itſelf, before it is diſſolv'd in *aqua fortis*. If the oil of tartar

tartar be poured on a ſolution of the copper, the *coagulum* is a pale green, which *coagulum* the ſpirit of ſalt diſſolves and leaves the liquor clear, but green, as before precipitation.

Tin-glaſs (an imperfect metal) diſſolv'd in *aqua fortis*, and mix'd with the *lixivium* of blood, produced a milky *coagulum*, and by the addition of the ſpirit of ſalt, after ſtanding ſometime, its upper ſurface changed to a light blue, both the *lixivium* of fleſh and the oil of tartar produced white *coagula*, which the ſpirit of ſalt ſcarcely alters.

Lead diſſolv'd in ſpirit of vinegar produces much the ſame white *coagulum*, when mix'd either with the *lixivium* of blood, fleſh, or the oil of tartar; nor does the ſpirit of ſalt make any alteration.

By all theſe experiments it is pretty evident, that none of theſe metalline bodies were affected by the *lixivium* of blood, ſo as to produce this fine blue. The two metals untried are gold and tin; the latter of which, when diſſolv'd in ſpirit of vinegar, has ſo near a reſemblance to lead, diſſolv'd in the ſame *menſtruum*, that in all probability the experiments would anſwer much alike in both. What may be expected from gold, Mr. *Brown* was not hitherto ſo well aſſur'd of; as he was from iron; which, when diſſolv'd in ſpirit of vitriol, will anſwer all the experiments that have been tried with the ſolutions of vitriol, and produce as fine a colour; nor can this be owing to any property in the diſſolvent itſelf, which, tho' drawn from the ſame kind of vitriol all along made uſe of in theſe experiments; yet is ſo alter'd by the violent fire in the production of it, as not to anſwer in any trials to the vitriol itſelf.

May we not, therefore, hence conclude, that iron is the ſubject of this beautiful colour, produced by means of the *lixivium* with blood.

The Remainder of the Bills *of* Mortality, *&c. of the ſeveral Towns in* Europe, *extracted from the* Acta Breſlavienſia; *by Dr.* Sprengell. Phil. Tranſ. N° 381. p. 25.

A Liſt of thoſe chriſtened and buried in Breſlaw *from the* 25*th. of* December 1719. *to the* 24*th of* December 1720, *incluſive.*

Buried.	
From the 25th of December, to the 31ſt,	40
In January	160
February	107
March	139
April	192
May	158
June	120
July	131
Auguſt	182
September	189
October	130
November	132
December, only till the 24th	127
	1816

Chriſtened.		
Males		564
Females		556
		1120

Married 460 pair.

Among the dead were

Married men		385
Married women		186
Widows and widowers		285
Batchelors		113
Maidens		113
Children to ten years of age.	Boys	345
	Girls	300
Stillborn	Boys	53
	Girls	30
		1810

In the year 1720 in Lignitz were

Married	92 pair.
Chriſtened	283
Buried	366
Among which were children.	149

In Jauer.

Chriſtened of Proteſtants	100 Ch.
Buried	175

In Oels.

Chriſtened	180
Buried	203

In Schweidnitz.

Married	218 pair.
Chriſtened	920
Buried	250

In Vienna.

From the 1ſt of *January* to the 31ſt of *December*.

Buried	6825
Chriſtened	4126

In Lobau.

Married	58 pair,
Born	160
Buried	355

Amongst which were born 80 boys and 80 girls: Buried 95 men and 48 women, besides 4 who died in childbed: 26 batchelors and 22 maidens: 36 boys and 42 girls; 25 chrisoms: 7 still-born: 55 widows and Widowers. Likewise 51 persons between 60 and 70 years of age: 29 between 70 and 80: 7 between 80 and 90: 1 of 90, and 1 of 99.

In Dresden.

Married 368 pair.
Born 1448
Among which were 719 boys, and 641 girls, legitimate: Illegitimate boys 44, and girls 44.
Buried 1733
Amongst which were 255 married men, and 182 married women, 189 widowers and widows: 88 young men and 84 young or single women: 883 children, *viz.* 461 boys and 422 girls, amongst which were still-born 72, *viz.* 37 boys, and 35 girls.

In John-George *city*.

Married 24 pair.
Christened 148
i. e. 79 boys and 69 girls, among which were 2 still-born, and 10 bastards.
Buried 243
viz. 37 married men and 18 married women: 5 widowers and 37 widows, 6 batchelors, and 18 maidens: 6
boys, 58 girls, &c.

In St. Annaberg.

Married 18 pai
Born 105
i. e. 63 boys and 42 girl among which were 2 pa of twins, and 4 bastards.
Buried 180
viz. 96 men and 84 women among which were 34 ma ried men and 18 marrie women, 1 in childbed; widowers and 28 widow 12 batchelors, and 17 ma dens: 38 boys and 20 girl among which was 1 dea born.

In Schneeberg.

Married 12 pai
Born 89
As 44 boys: 45 girls; amon which were 4 still-born, an 3 bastards.
Buried 157
viz. 28 married men, and married women: 1 chil bed woman: 5 widowe and 25 widows: 9 batchelo and 10 maidens: 68 chi dren, *viz.* 49 boys and girls.

In Leipsic.

Married 314 pa
Born 790 *vi*
405 boys and 385 girls.
Buried 1264
Among which were 233 m and 146 women: 86 batch lors, and 62 maidens: 2 boys and 195 girls: ni wom

women in childbed: 98 chrisoms, *i. e.* 53 boys and 45 girls: 58 still-born, as 36 boys and 22 girls.

In Berlin.

Married	669 pair.
Born	2279
Buried	2426

In Wismar.

Christened	168
Buried	100

In Epperies.

Born	171
Buried	116

In Rawitz.

Born 134

And just as many males as females, except two pair of twins.

Buried 95

Among which were 15 childbed women, and 3 still-born.

In Dantzick.

Born	1862
Buried	1610
Married	442

This is merely of the city, and not of the suburbs of *Dantzick*.

A List of the several Cities and Towns in the Kingdom of Prussia.

In the Year 1720.	*Christened.*	*Pair marr.*	*Buried.*
In Angerberg	535	88	259
Balga	638	113	395
Bartenstein	224	63	188
Barthen	337	66	246
Brandenburg	936	213	576
Teutsch-Eulau	100	25	82
Fishausen	478	103	270
Gerdauen	489	98	206
Gilgenburg	225	56	137
Insterburg	2386	336	1398
Johannisburg	378	68	188
Labiau	616	92	273
Liebstadt	440	73	343
Lotzen	256	45	148
Luck	364	65	174
Marienwerder	769	145	641
Mummel	953	168	448
Neydenburg	512	104	437
Neuhausen	243	62	141

	Christened.	*Pair marr.*	*Buried*
In Neuhoff	28	6	10
Oletzko	688	111	26
Ortelsburg	476	82	33
Osterode	784	92	30
Preuss Eulau	479	112	28
——— Marck	705	130	47
——— Holland	705	125	55
Ragnit	930	93	51
Rastenburg	610	155	45
Rhein	482	89	19
Schacken	576	96	25
Schonberg	276	51	23
Sehesten	354	62	18
Taxium	876	157	44
Tilsit	1139	187	64
The City of Koningsberg	1682	474	140

In the year 1721. In *Breslaw*

Buried.	
From the 25th of December to the 31st 1720.	38
January, 1721.	123
February	129
March	161
April	127
May	149
June	132
July	86
August	103
September	125
October	99
November	121
December till the 24th,	89
Total	1482

Christened.	
Males	61
Females	58
Total	119
Married	405 pa
Among the dead were.	
Married men	30
——— Women	15
Widows and widowers	20
Batchelors	9
Maidens	8
Children to ten years of age: Boys	32
Children to ten years of age: Girls	24
Stillborn: Boys	4
Stillborn: Girls	2
Total	148

In Jauer.

Buried 114 Christened 124

In Vienna.

Buried 6490 Christened 4104

So that there died 2386 more than were christened.

Among the dead were 43 casualties.
Besides 8 persons of 90 years of age

1		91
2		92
3		93
3		94
3		96
1		98
4		99
2		100
1		105

In Dresden.

Buried 1850 Christened 1396 Married 404 pair.

Amongst the buried were.			
Married men	274	Girls	400
—— women	206	Still born { Males	50
		Still born { Females	28
Widowers	42	Among the baptiz'd were,	
Widows	238	Boys	701
Batchelors	128	Girls	690
Maidens	93	Bastards { Males	40
Boys	479	Bastards { Females	51

In LEIPZICK

Buried.

	Married men	Married women.	Batchelors.	Maidens.	Boys.	Girls.	Childbed women	Chrysoms. Males.	Chrysoms. Females	Still-born. Males.	Still-born. Females.	Widowers & wid.	Summa.
January	28	25	13	11	15	14	1	1	1	2	3	20	134
February	17	12	15	9	16	10	2	4	3	7	2	17	114
March	28	29	13	12	14	15	0	1	3	3	3	17	138
April	31	24	9	10	17	22	2	2	3	2	0	13	135
May	25	23	10	9	23	16	3	6	5	0	0	16	136
June	28	16	17	4	15	13	2	3	6	2	2	12	120
July	7	10	7	5	8	12	2	1	4	0	3	15	74
August	14	8	6	6	19	11	3	2	3	3	2	9	86
Septem.	14	14	1	2	13	18	2	4	4	1	1	7	81
October	17	10	4	7	14	20	6	7	2	7	4	5	103
Novemb.	12	13	3	6	20	7	1	3	7	2	0	11	85
December	16	12	2	12	19	17	10	5	3	2	1	7	94
Summ.	237	169	100	93	193	175	24	37	44	31	21	149	1300

Among the dead were
218 from 60 to 69 years
82 70 79 old.
16 80 88
2 90 91

Besides 1 batchelor of 60, and 2 of 61; and 5 maidens of 60, 62, 66, 70, and 73 years of age.

Christened.

	Males.	Females.	Total.	Pair marr.
January.	43	31	74	13
February.	31	23	54	30
March.	34	31	65	1
April.	32	38	70	
May.	31	22	53	20
June.	21	31	52	24
July.	37	26	63	21
August.	27	38	65	17
Septem.	29	46	75	30
October.	38	33	71	20
Novem.	37	34	71	52
December	27	20	47	1
Sum.	387	373	760	268

Among the christened were. Eight posthumous; 14 twins; 63 bastards; and 2 *Jews* baptized.

So that in this year there died 1300. Born and christened 760. Hence there are 540 fewer born than died.

In the *Marck*.

Christened 16086 Amongst which were 596 bastards.
Married 4613 pair.
Buried 13511

More born 2575 than buried.

Among the buried were 28 persons of 90 years of age and upwards, besides 3 of 100, 1 of 101, 1 of 102, 2 of 104, 2 of 107, and 1 of 112 years.

In the whole *Royal Prussia*.

Born 75275. Buried 58017. That there are 17258 more born than buried.

Next

Next following the Special List of the Kingdom of Pruss[illegible]

	Christened.	Pair Marr.	Buried
In Angerberg	518	104	28
Balga	605	168	38
Bartenstein	278	65	21
Bahrten	357	64	20
Brandenburg	944	222	69
Dutch-Elau	103	20	7
Fishausen	498	112	30
Gerdauen	490	75	21
Gilgenburg	110	37	7
Insterburg	2235	359	88
Johannisburg	463	76	17
Labiau	569	92	27
Liebstadt	431	94	25
Lotzen	247	68	18
Lyck	352	96	18
Marienwerder	765	162	40
Mummel	914	193	52
Neydenburg	545	121	32
Neuhausen	234	59	13
Neuhoff	34	10	4
Oletzko	631	103	31
Ortelsburg	497	92	28
Osterode	369	144	18
Preuss Eylau	503	113	42
—— Holland	605	166	37
—— Marck	657	134	33
Rangnit	791	95	33
Rastenburg	738	165	53
Rhein	416	108	21
Shacken	514	87	32
Schonberg	255	61	14
Schesten	330	80	23
Taxium	870	159	50
Tilsit	1245	185	58
the City of Koningsberg	1655	424	177
Total	20768	4313	1240

In *Ratisbon*

Christened Males --- 150
Females -- 120

Total 250

Amongst which were 3 pair of twins, *viz.* 2 boys and girls. Besides 6 bastards, *i. e.* 3 boys and 3 girls. Buried 220. *viz.* Married men 41. Married women 43. Young men 11. Young women 15. Children 110. *i. e.* 67 boys and 43 girls. Amongst which, widows 23. Lying-in women 2. Stilborn 2. Married 67 pair.

In *Nurnburg.*

Buried		Christened.	
Married Men	234	Males	541
—— Women	257	Females	543
Bachelors	48		
Maidens	73	Total	1084
Boys	232		
Girls	191		
Stilborn	18		
In the Suburbs	10		
Total	1063		

21 more born than died, amongst the christened were 16 pair of twins.

In *Copenhagen.*

Born 2630 Buried 2247.

In *Amsterdam.*

The following list contains 7 years, *viz.* from 1715 to 1721.

Anno 1715	died	7633	Persons
16		7078	
17		7451	
18		8644	
19		9726	
20		7820	
21		7632	

In *Epperies.*

Born 214. Buried 142.

In *Dantzick.*

Born 1833. Buried 1435. Married 457 Pair.

An Account of the Dissection of an Eye *with a* Cataract; *by Mr.* John Ranby. Phil. Trans. N° 381. p. 36.

SEPT. 21, 1723. Mr. *Ranby* was consulted by one *William Sollars*, 50 years of age, who complained of a decay in his sight: Upon examining his eyes, he observed two cataracts, that in his right eye almost ripe, the other just forming. There being no other obvious method of relieving the patient, Mr. *Ranby* proposed the operation; but first advised him to consult some others of the profession; and accordingly advising with Mr. *Tanner*, he, upon viewing his eyes, declared them both cataracts, but that neither of them was yet fit for the operation. In the mean time the patient fell ill of a fever, and died the second of *March*. Mr. *Ranby* procured the right eye, in which the cataract was most confirmed, in order to make an exact dissection of it: In examining it, he found the aqueous and vitreous humours in their natural state; but the crystalline humour opaque and of a foul pearl colour, and more solid substance than usual. The generally received opinion, that a cataract is a film, growing behind or sometimes before the pupil, made Mr. *Ranby* examine both chambers of the aqueous humour, with the utmost exactness, but all to no purpose; for the aqueous humour had its natural transparency, nor could he observe any thing preternatural, either on the *iris* or *uvea*, excepting too great a contraction of the pupil. This very much confirms the opinion of *Maitrejean*, *Brisse*, *Heister* and *Valsalva*, who have severally asserted, that a cataract is only an opacity of the crystalline humour; and that it naturally proceeds from a serous acid, which so far astringes, and corrodes its substance, as to destroy its transparency. This *Maitrejean* confirms by an experiment of immerging the crystalline humour in a composition of three parts water and one of *aqua fortis*, by which he tells us it may be rendered hard and opaque: But in this point Mr. *Ranby* is of the same opinion with the learned Dr. *Pitcairn*, who has sufficiently proved, that there is no such serous acidity in an animal body. To Mr. *Ranby* nothing seems more easy than to deduce this opacity of the crystalline humour from an inflammation in the blood, or an increased *momentum* in the fluids, with which it is supplied: For, in that case grosser particles, inconsistent with the transparency, may be impelled into the lymphatic vessels, of which it is composed; and that there is an inflammation is sufficiently demonstrable hence, 1. The patient often feels a pungent pain in the eye, which, as it

it is generally the forerunner of a cataract; so it certainly indicates an inflammation of the part. 2. These *maculæ*, which appear, swimming in the air, as it were, plainly prove, that there are opaque particles already entered into the lymphatic vessels, which compose the vitreous humour. 3. The *iris*, whose colour arises from the blood-vessels, as it changes from a lighter to a darker colour, shews the violence of the inflammation; and is, therefore, esteemed a symptom of the worst consequence.

Observations on the Comet, *that appeared in* October, November *and* December 1723; *by Mr.* Bradley. Phil. Trans. N° 282. p. 41.

THE small comet, seen in these parts of *Europe* in *November*, *October* and *December* 1723, was first observed in *England* by Dr. *Halley October* 9, between seven and eight o'clock in the evening; it appearing then to the naked eye not much unlike a star of the third magnitude: Viewing it thro' a telescope, he saw some small telescopic stars near it, whose situation he noted together with that of the comet, in order to see which way it tended. About 9 o'clock he again viewed the comet, and found it considerably moved from its former station, having now passed a small star, which at the time of the first observation was on the other side of it: Comparing the two situations of the comet together, he perceived, that its apparent motion at that time was about 8 or 9 minutes in an hour, in a direction towards *sagitta*; and that the comet passed very near it, if not entirely eclipse the abovementioned small star, whose place he afterwards found to be in 7° 22′ 15″ of *Aquarius*, with 5° 2′ N. Lat. From the situation of the comet at the time of the first observation, he judged it was in conjunction with the star at 8^h 5′ equal time.

Note, that the equal (and not the apparent) time, is likewise made use of in all the following observations.

October 10. Dr. *Halley* having communicated to Mr. *Bradley* the substance of what he had observed, the latter was enabled the night following to see the comet at *Wansted*: The clouds hindered him from observing it in the manner he designed; but he had time enough to measure its distance (with a micrometer in a 7 foot telescope) from a star in *Aquarius*, marked ε by *Bayer*. At 6^h 21′ the observed distance between this star and the comet was 1° 13′ 53″, and a great circle passing thro' the star and comet made an angle with the vertical circle of 60° and ¼. The

$\frac{1}{4}$. The comet was more southerly and westerly than the star. By this observation the comet preceeded the star 1° 3′ 50″ *R. Ascen.* being 39′ 5″ more southerly: So that the comet's *R. Ascen.* was 307° 6′ 40″ and its declination 11° 8′ 15″ S.

The place of ε here assumed is according to the *Catal. Brit.* as are also the places of the other stars hereafter mentioned, from which the comet was observed. The right ascensions and declinations, here set down, of the several small stars, that are not in that catalogue, were determined by observing the differences of *R. Ascen.* and declination between those small stars and others that were in the catalogue, and had nearly the same declinations.

The same evening at 7^h 3′ a small star that was more easterly than the comet, and had much the same declination with it, was distant from it 35′ 40″. About the same time another small star, that had nearly the same *R. Ascens.* with the comet, but was more southerly, was distant from it 39′ 58″. The places of these two stars he had not hitherto observed.

The next night proved cloudy; so that Mr. *Bradley* could not see the comet again till *October* 12. when, the air being very serene and clear, he (Mr. *Pound* assisting in this and the following nights observations) had an opportunity of comparing it with two or three small stars, that were near it.

At 7^h 22′ a small star, whose right ascension was found 304° 40′ 23″, and its declination 7° 8′ 22″ S. preceeded the comet in *R. Ascen.* 26′ 21″ being 10′ 42″ more northerly: Hence the comet's *R. Ascen.* was 305° 6′ 44″ and its declination 7° 19′ 4″ S.

At 8^h 50′ the comet was in the same parallel of declination with another small star, whose *R. Ascen.* was found 305° 9′ 56″ and its declination 7° 13′ 20″ S. and preceeded the said star 6′ 20″ *R. Ascen.* Hence the *R. Ascen.* of the comet was 305° 3′ 36″ and its declination 7° 13′ 20″ S. These observations were made with a 15 foot telescope, furnished with a micrometer; as were also all those of the following nights.

The next night, *viz. October* 13, 6^h 58′ the comet followed a small star 4′ 10″ *R. Ascen.* being more northerly than the star 11′ 45″. The clouds did not permit to observe the place of this star; but its right *Ascen.* must be about 304° 22′ and its declination 6° 10′ S.

October 14. The comet was near 2 stars, which are the 66th and 67th of *Aquila* and *Antinous* in the *Catal. Brit.* and at 8^h 57′ it followed the southermost of them 20′ 37″ *R. Ascen.* being

being 29′ 8″ more ſoutherly: Hence the comet's *R. Aſcen.* was 303° 49′ 10″ and its declination 4° 43′ 54″ S.

October 15. 6h 35′ the comet preceeded the northermoſt of the ſaid ſtars 23′ 6″ *R. Aſcen.* being more ſoutherly than the ſtar 4′ 15: Hence the *R. Aſcen.* of the comet was 303° 24′ 40″; its declination 3° 51′ 3″ S.

October 21. 6h 22′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 7′ 17″ and its declination 11′ 50″ S.) preceeded the comet 41′ 6″ *R. Aſcen.* being 5′ 50″ more ſoutherly. Hence the comet's *R. Aſcen.* was 301° 48′ 23″ and its declination 6′ S.

October 22. 6h 24′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 39′ 47″ and declination 32′ 43″ N.) followed the comet ½ a minute *R. Aſcen.* being 13′ 43″ more northerly: Hence the comet's *R. Aſcen.* was 301° 39′ 17″ and its declination 19′ N.

October 24. 8h 2′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 24′ 57″ and its declination 1° 9′ 22″ N.) preceeded the comet 37″ *R. Aſcen.* being 5′ 12″ more northerly: Hence the comet's *R. Aſcen.* was 301° 25′ 34″ and its declination 1° 4′ 10″ N.

October 29. 8h 56′ a ſmall ſtar (whoſe *R. Aſcen.* was found 301° 6′ 20″ and declination 2° 51′ N.) preceeded the comet 1′ *R. Aſcen.* being 23′ 40″ more northerly: Hence the comet's *R. Aſcen.* was 301° 6′ 20″ and its declination 2° 27′ 20″ N.

October 30. 6h 20′. The ſame ſtar had exactly the ſame *R. Aſcen.* with the comet, being 11′ 33″ more northerly: Hence the comet's *R. Aſcen.* was 301° 6′ 20″ and its declination 2° 39′ 27″ N.

November 5. 5h 53′. A ſmall ſtar (whoſe *R. Aſcen.* was found 300° 35′ and its declination 3° 45′ 30″ N.) preceeded the comet 33′ in *R. Aſcen.* being 2′ 8″ more ſoutherly: Hence the comet's *R. Aſcen.* was 301° 8′ and its declination 3° 47′ 38″ N.

November 8. 7h 6′. A bright ſtar placed by *Hevelius* in *Roſtro Aquilæ*, but not inſerted in the *Catal. Brit.* (whoſe *R. Aſcen.* at this time was found 302° 21′ 30″ and declination 4° 28′ 4″ N.) followed the comet 1° 7′ 40″ in *R. Aſcen.* being 13′ 3″ more northerly: Hence the comet's *R. Aſcen.* was 301° 13′ 50″ and its declination 4° 15′ 37″ N.

November 14. 6h 20′. A ſtar (whoſe *R. Aſcen.* was found 301° 27′ 10″ aud its declination 4° 59′ 40″ N.) preceeded the comet 5′ 35″ in *R. Aſcen.* being 5′ 50″ more ſoutherly: Hence the comet's *R. Aſcen.* was 301° 32′ 45″ and its declination 5° 5′ 30″ N.

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This was the laſt time Mr. *Bradley* obſerved the place of the comet, till after the full moon.

Dr. *Halley* and Mr. *Graham* continued to obſerve the comet till *November* 20. and according to both their obſervations that evening at $7^h\ 45'$ the comet followed β in *Collo Aquilæ* $6^\circ\ 33'\ 55''$ in *R. Aſcen.* being about $4'$ more northerly than the ſtar: Hence the comet's *R. Aſcen.* was $301^\circ\ 59'\ 50''$ and its declination $5^\circ\ 48'\ 55''$ N.

The light of the moon increaſing daily, prevented them from making any more obſervations, the comet being by this time grown ſo faint, as to become in a manner imperceptible, while the moon ſhine bright: And the faint appearance it made before the moon obſtructed the ſight of it, gave little hopes of its being ſeen again after the full moon. Notwithſtanding which Mr. *Bradley* (being then near *Cirenceſter* in *Glouceſterſhire*) was tempted by the ſerenity of the evening, and the uſe of a very good 10 foot teleſcope, to look for it again before the moon roſe; and he found it among ſome ſmall teleſcopic ſtars; but it appeared ſo faint and dull, as made it doubtful, whether what he took for the comet might not be a ſmall ſtar with a little hazineſs about it. But this doubt was cleared two nights after, when he perceived that the comet was moved from its former ſituation, towards a bright teleſcopic ſtar, from which he afterwards took its difference of *R. Aſcen.* and declination upon his return to *Wanſted* on *December* 7.

This ſtar's *R. Aſcen.* was then found to be $303^\circ\ 39'\ 20''$ and declination $7^\circ\ 32'\ 30''$ N. And *December* 7. $6^h\ 45'$ the comet followed it $3'\ 15''$ in *R. Aſcen.* being $14'$ more northerly than the ſtar: Hence the comet's *R. Aſcen.* was $303^\circ\ 42'\ 35''$ and its declination $7^\circ\ 46'\ 30''$ N.

This was the laſt night Mr. *Bradley* ſaw the comet, an uninterrupted ſucceſſion of cloudy evenings ſo long prevented his obſerving it, that it became uncertain where to look for it.

The abovementioned obſervations are the principal of thoſe made at *Wanſted*; and moſt of them being taken from ſtars, which are not in the *Britiſh* catalogue, whoſe places, therefore, are here determined, only by comparing them with ſome that were; it cannot be ſuppoſed, that the comet's places, deduced from them, are altogether exact. For which reaſon Mr. *Bradley* has all along ſet down not only the place of the comet and ſtar, where it was known; but alſo the particulars of the obſervation, that if any hereafter ſhould be willing to examine the track of this comet more nicely, they may know where to find

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the ſtars, from which it was obſerved. The places of the ſtars here ſet down are abundantly ſufficient for that purpoſe, as will appear from the following table, which contains the longitudes and latitudes of the comet, deduced from the foregoing obſervations; together with the places of the comet, calculated from the theory of gravity, for the times of obſervation on the ſeveral days therein mentioned; as alſo the differences between the obſerved and computed places. Thoſe differences, not exceeding 1 minute, ſhew, that the obſervations are not only conſonant to each other; but that the places of the ſtars are likewiſe near the truth; ſince the comet's places, deduced from them, are found all along to agree ſufficiently near with the theory of gravity; the truth of which having long ſince been eſtabliſhed by its great author Sir *Iſaac Newton* and by Dr. *Halley*, needs not the confirmation of ſo ſhort a ſeries of obſervations, as was made of this comet: But ſhort as it is, Mr. *Bradley* preſumes, it will be no eaſy matter to account for the obſervations with the ſame degree of exactneſs any other way than by that theory, according to which the following computations are made.

1723. equat. time				Long. of comet obs.				N. Lat. observed			Long. of comet comput.				N. Lat. computed			Differ. of Long.		Differ. of Lat.	
	d.	h.	′		°	′	″	°	′	″		°	′	″	°	′	″		″		″
Octob.	9	8	5	♒	7	22	15	5	2	0	♒	7	21	26	5	2	47	+	49	−	47
	10	6	21		6	41	12	7	44	13		6	41	42	7	43	18	−	30	+	55
	12	7	22		5	39	58	11	55	0		5	40	19	11	54	55	−	21	+	5
	14	8	57		4	59	49	14	43	50		5	0	37	14	44	1	+	48	−	11
	15	6	35		4	47	41	15	40	51		4	47	45	15	40	55	−	4	−	4
	21	6	22		4	2	32	19	41	49		4	2	21	19	42	3	+	11	−	14
	22	6	24		3	59	2	20	8	12		3	59	10	20	8	17	−	8	−	5
	24	8	2		3	55	29	20	55	18		3	55	11	20	55	9	+	18	+	9
	29	8	56		3	56	17	22	20	27		3	56	42	22	20	10	−	25	+	17
	30	6	20		3	58	9	22	32	28		3	58	17	22	32	12	−	8	+	16
Nov.	5	5	53		4	16	30	23	38	33		4	16	23	23	38	7	+	7	+	26
	8	7	6		4	29	36	24	4	30		4	29	54	24	4	40	−	18	−	10
	14	6	20		5	2	16	24	48	46		5	2	51	24	48	16	−	35	+	30
	20	7	45		5	42	20	25	24	45		5	43	13	25	25	17	−	53	−	32
Dec.	7	6	45	♒	8	4	13	26	54	18	♒	8	3	55	26	53	42	+	18	+	36

In order to determine the orbit of this comet, Mr. *Bradley* supposed it to describe a parabola, agreeable to what is delivered in *lib.* 3. of Sir *Isaac Newton's Princip. Math.* and then he found the inclination of the planes of the orbit and ecliptic 49° 59′; the place of the ascending node 14° 16′ of *Aries*; the place of the perihelion 12° 52′ 20″ of *Taurus*; the distance of the perihelion from the node 28° 36′ 20″; the logarithm of the perihelion distance 9.999414; the logarithm of the diurnal motion 9.961007; the time of the comet's being in its perihelion *September* 16. 16^h 10′ equal time. The motion of the comet in its orbit, thus situated, was retrograde or contrary to the order of the signs.

From these elements, by the help of Dr. *Halley*'s general table for comets, (to which they are adapted) Mr. *Bradley* computed the places in the foregoing table, which agreeing with the observed places as near as the observations themselves agree with each other, shew, that it would be a vain attempt to pretend to determine the true ellipsis in which this comet moves, or its periodical revolution, from so small a part of its orbit as it described between the first and last of the foregoing observations: This, therefore, must be left to posterity; especially, since it is certain, that this comet is not one of those of which observations have hitherto been transmitted to us, sufficient to determine the situation of their orbits.

The *nucleus* of this comet was very little; for, it appeared but of a small diameter, when Mr. *Bradley* first saw it; tho' it was then above three times nearer the earth than the sun is at its mean distance: Its tail was then hardly discernible with the naked eye; but thro' a telescope one might perceive a faint light, extending itself above a degree from the body.

Mr. *Bradley* had not hitherto heard, that this comet was seen before *October* 6. tho' it was in a proper situation to have been observed in the morning, the most part of *September*; especially, from the time it was in its perihelion, till near the end of that month: For, about that time it crossed the *milky way* between the *mast of the ship* and the head of the *Great dog*, passing between the bright stars in the body and tail of the *Great dog*, towards the head of the *dove*, where it was about *September* 29. being by that time got so far towards the south-pole, as not to rise above our horizon. From thence it passed under the tail of *Xiphias* within about 15° of the south pole of the ecliptic; and moving on between the head of *hydrus* and the bright star in *Eridanus*, called *Acharnar*, it went by the stars in

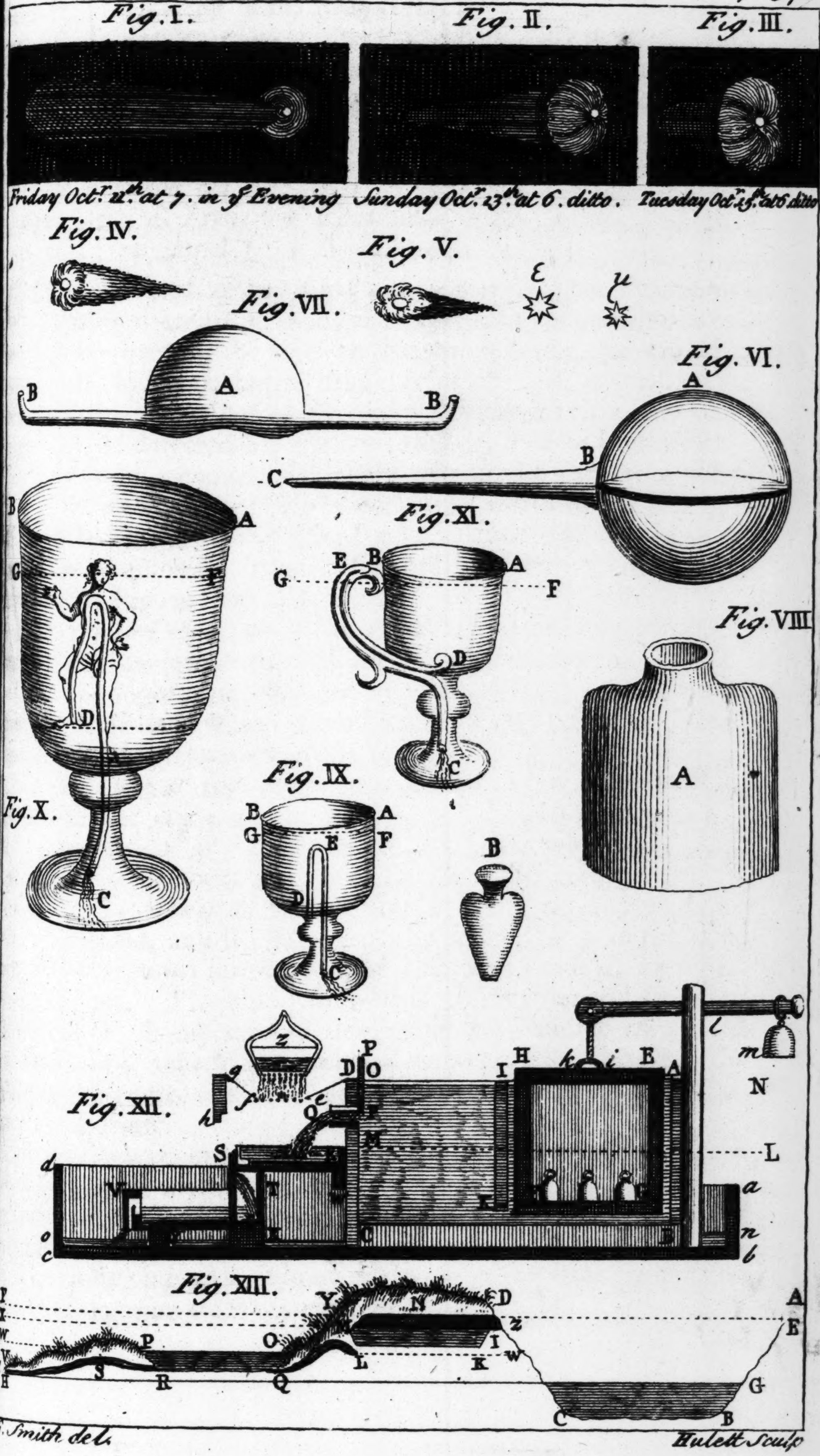
in the body and neck of the *crane* about *October* 5. when it came again above our horizon: From hence passing under the tail of the southern *fish*, and between the stars in the shoulder of *Capricorn*, it crossed the ecliptic *October* 8. in about 8° and ½ of *Aquarius*: From thence it moved on by the hands of *Aquarius* and *Antinous* towards the head of the *eagle*, according to its course before described.

The comet was in opposition to the sun *October* 1. when it had near 74° S. Lat. and altered its longitude 2 signs in a day. About *October* 3. it was in its *perigæum* or nearest distance to the earth, being then almost 10 times nearer it than the sun is at its mean distance; and its apparent motion was then about 20° in a day; and when Mr. *Bradley* last saw it, it was above twice as far off as the sun.

Observations on the same Comet *at* Witham *in* Essex; *by the Lord* Paisley. Phil. Trans. N° 382. p. 50.

HIS Lordship being at *Witham* in *Essex*, where he had the advantage of a very clear sky, first discovered this comet on *Friday* the 11th of *October*, 1723, about 7 o'clock in the evening; it then appeared not much unlike a star of between the fourth and fifth magnitude; but a haziness round the head, and some light streaming from it on that side, opposite to the sun, induced him immediately to look upon it as a small comet; which his observation next evening abundantly satisfied him of. He was very particular in the notice he took of its appearance, Fig. 1, 2, 3. Plate XI. representing it on three several nights, *viz.* the 11th 13th and 15th of the same month: Some time after the tail became so inconsiderable, as hardly to deserve any farther description, as will be readily judged from the decrease of it between the 11. and 15. days of the month. The tail was visible on the 11. to near a degree's distance from the body, as he found by comparing it with some known distances in the heavens; it was of a dusky light, not unlike a cloud growing darker and darker towards its extremity, as represented in Fig. 1. where, as well as in the two following Fig. the whole speck in the head is designed to shew the brightness of a small star; from the comparison of which with the tail, the brightness of the latter may in some sort be collected. The tail appeared sharper and not so much spread in the two following observations, and in the last did not exceed ⅓ part of the first length; it was then of a much darker colour, which made the difference between that and the head more observable, the

head



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head still appearing sufficiently bright. For some following nights his observations were interrupted by cloudy weather, after which the comet was so far diminished, as only to be known by its motion, its appearance being no ways distinguishable from that of a small nebulose star.

An Observation on the same Comet *at* Albano October 1723; *by S.* Francisco Bianchini. Phil. Transf. N° 382. p. 51. *Translated from the* Latin.

OCTOB. 17. S. *Bianchini*, after observing the situation of *Jupiter's* satellites, chanced to cast his eyes on *Capricorn*; and running over all the stars in that constellation, he espied a certain nebulose star, larger than any of the rest, but which he never observed there before; In order to examine this new phænomenon, he directed his telescope thither, and he immediately found it to be a comet; for, there appeared a very thin nebulous globe with a small bright *nucleus* in the middle: It might likewise be seen with the naked eye. Besides the *nebula* or atmosphere of a comet, it had a short tail, regarding the east, as represented Fig. 4. Plate XI.

S. *Bianchini* made his usual observations on this comet, in order to find its longitude, latitude and proper motion.

The first night, *viz.* *October* 17. about $7^h\ 44'$ *p. m.* it pass'd over the meridian (which almost coincides with that of *Rome*) and was distant from the zenith $69°\ 29'$.

At $8^h\ 11'\ 30''$ the comet was distant from *Fomahant* of *Aquarius* $20°\ 33'$; and at $8^h\ 17'\ 30''$ from β in *Aquarius's* right shoulder $21°\ 8'$; consequently, the comet was in $11°\ 54'$ of *Aquarius*, with about $11°\ 10'$ S. Lat. from the ecliptic.

On the 21. it was as near the star ε in the net above *Aquarius's* left hand, as ε itself is near the lesser star μ in the same net, and the comet formed a right line with ε and μ, as represented Fig. 5. Therefore, from this observation and from the difference of *R. Ascen.* and declination between the comet and the said star ε, which S. *Bianchini* very carefully observed, the place of the comet was in $6°\ 45'$ of *Aquarius* with $8°\ 5'$ N. Lat. from the ecliptic.

Hence likewise may be inferred the proper motion and path of the comet; which was thro' the plane of a great circle, intersecting the ecliptic in $9°$ of *Aquarius*, and forming with the ecliptic an angle of $80°$ nearly.

The following days it moved always in the same proportion and its elongation from the earth encreased daily.

After ſeveral trials he could find no ſenſible parallax; conſequently, its diſtance from the earth muſt be very conſiderable.

As to the comet's latitude there was not above a minute or two difference between *Bianchini*'s obſervation, and that made by F. *Carbone* and *Capaſſo*; in all other reſpects the obſervations perfectly agreed.

Obſervations about Waſps *and the Difference of their* Sexes; *by Mr.* Derham. Phil. Tranſ. Nº 382. p. 53.

IN *July* 1723. Mr. *Derham* obſerved ſeveral waſps flying about the top of the collegiate *Chapel* in *Windſor Caſtle*, and particularly, frequenting a covering of deal-boards and the pieces of timber, lying on the leads; moſt of theſe waſps were of a larger ſort than uſual, and he imagined they came thither to gnaw the wood, and carry it away in mouthfuls to build their neſts; of which he has taken notice in his *Phyſ. Theol.* particularly, *Book* 4. *ch.* 11. *note* 21. and *ch.* 13. *note* 12: On *July* 6. he obſerved a cluſter only of three waſps, cloſely embracing each other; one of which was a large female waſp; the other two of a leſſer ſort: Soon after he found 8 or 10 waſps, cloſely hanging together, and divers other ſuch like parcels: In the midſt of all which was conſtantly a queen-waſp, and only one; the reſt being always of a different ſort from either the queen or the common waſps, which made him ſuſpect they were male and female; and, therefore, examining another cluſter of them with greater exactneſs, he found the queen-waſp in *coitu* with one of the other waſps, and ſo cloſely joined tail to tail, that it was ſome time before they were parted.

After this he caught all the waſps he could on the top of the chapel, but he could not ſee one of the common labouring waſps among them; but all were for the moſt part male waſps, with now and then a queen or female among them, and ſhe commonly in *coitu*.

Hence it appears, that there are three ſorts of waſps; the queens or females, the kings or males and the common labouring waſps, and each of them very diſtinct.

The queen or female waſp (by many called the king-waſp) is much longer in the body and larger than any other waſp.

The male waſps are leſſer than the queens; but as much longer and larger than the common waſps, as the queen is longer and larger than theſe: The males have no ſtings as the queens and common waſps all have; and theſe are according to

Mouſet,

Moufet, what authors call Ἀκεντρυς, and take to be females; tho' he himself be of another opinion, imagining all wasps to have stings, from his examining a wasps nest at *Ham*, *Anno* 1587. in which he found none without a sting. But Mr. *Derham* is surprised, how that curious enquirer should miss of these sting-less male wasps: He must either have been too hasty in his examination, and not being aware of the difference, have thought the males (which are but few in number to the labouring wasps) were the same and had stings as well as the rest; or else he made his enquiry at a time, when perhaps the males had deserted the nest, which, probably, they may do, as the male or drone-bees are forced to do; or else the year 1587 (in which *Moufet* made his observation) might produce fewer wasps, at least fewer male-wasps than the summer of the year 1723 did, (in which Mr. *Derham* made his) which was observed to have a greater abundance of wasp-nests than had been known for many years: And in all the nests he searched into, he constantly found male wasps more or less, according to the size of the nest and number of wasps therein; and the part of the nest, where these males are bred, or at least where he found them mostly reside, was chiefly the two uppermost cells or partings between the combs, but one.

But to return to the distinction of the male wasps; another thing by which they may be known from other wasps, is their *antennæ* or horns, which are longer and larger than either those of the queen or common wasps; and with them they seem in running to feel more than the others do.

But the chief difference is in the parts of generation of these male-wasps, which are quite different from those of other wasps.

For the discovery of these parts, if the *alvus* be pressed, an horny or shell-like part will be thrust out, of a shining black colour, which consists of two parts like shells, somewhat resembling the castagnets used in dancing; at the extremity of each grows a hook, somewhat like those of the earwig's tail, but smaller; in the middle between these hooks appear three parts, the middlemost of which is a stiff brown tube, very curiously made with the fore-part like a spoon or ladle, and the other end within the body, is neatly branched and braced to each side within the two shells abovementioned: A little above which branching is a φυματιον or swelling, like that of a dog's pizzle; and probably, serves for the same use, if this tube be (as Mr. *Derham* imagines) the *penis* of the wasp.

On

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Hence it appears, that there are three ſorts of waſps; the queens or females, the kings or males and the common labouring waſps, and each of them very diſtinct.

The queen or female waſp (by many called the king-waſp) is much longer in the body and larger than any other waſp.

The male waſps are leſſer than the queens; but as much longer and larger than the common waſps, as the queen is longer and larger than theſe: The males have no ſtings as the queens and common waſps all have; and theſe are according to *Mouſet*,

Moufet, what authors call Ἀκεντρυς, and take to be females; tho' he himself be of another opinion, imagining all wasps to have stings, from his examining a wasps nest at *Ham*, *Anno* 1587. in which he found none without a sting. But Mr. *Derham* is surprised, how that curious enquirer should miss of these sting-less male wasps: He must either have been too hasty in his examination, and not being aware of the difference, have thought the males (which are but few in number to the labouring wasps) were the same and had stings as well as the rest; or else he made his enquiry at a time, when perhaps the males had deserted the nest, which, probably, they may do, as the male or drone-bees are forced to do; or else the year 1587 (in which *Moufet* made his observation) might produce fewer wasps, at least fewer male-wasps than the summer of the year 1723 did, (in which Mr. *Derham* made his) which was observed to have a greater abundance of wasp-nests than had been known for many years: And in all the nests he searched into, he constantly found male wasps more or less, according to the size of the nest and number of wasps therein; and the part of the nest, where these males are bred, or at least where he found them mostly reside, was chiefly the two uppermost cells or partings between the combs, but one.

But to return to the distinction of the male wasps; another thing by which they may be known from other wasps, is their *antennæ* or horns, which are longer and larger than either those of the queen or common wasps; and with them they seem in running to feel more than the others do.

But the chief difference is in the parts of generation of these male-wasps, which are quite different from those of other wasps.

For the discovery of these parts, if the *alvus* be pressed, an horny or shell-like part will be thrust out, of a shining black colour, which consists of two parts like shells, somewhat resembling the castagnets used in dancing; at the extremity of each grows a hook, somewhat like those of the earwig's tail, but smaller; in the middle between these hooks appear three parts, the middlemost of which is a stiff brown tube, very curiously made with the fore-part like a spoon or ladle, and the other end within the body, is neatly branched and braced to each side within the two shells abovementioned: A little above which branching is a φυματιον or swelling, like that of a dog's pizzle; and probably, serves for the same use, if this tube be (as Mr. *Derham* imagines) the *penis* of the wasp.

On

On each side this *penis*, lye two stiff parts branched at the top, with somewhat like hairs, thereby resembling brushes; at the bottom of these are two curious black cells, with an opening on one side, like that of the *concha veneris*, and with small whitish hair growing on one edge thereof. What the use of these two brush-like members may be, Mr. *Derham* could not tell, unless it were to strengthen and support, or direct the *penis* in *coitu*, or provoke therein.

Behind all these parts, already described, and more within the body, lies a long contorted white vessel, which at first Mr. *Derham* took to be the real *penis*, penetrating the ladle-like tube already mentioned: But upon farther examination, he rather takes it to be the spermatic vessel.

The use of the 2 little hooks at the extremity of the *europygium* or shells, Mr. *Derham* takes to be, the catching hold of the female's *podex*, and directing and assisting the penetration of the *penis* in *coitu*.

As to the parts of generation in the queen or female-wasps, there was nothing to be seen so remarkable, as in the male; but those parts are very like what we see in the common labouring wasps: And with the most accurate observations Mr. *Derham* could make with his microscopes, he could not perceive any difference at all. For which reason he supposes it is, that most of the writers on wasps and bees have been very confused and wavering about the sexes of these two tribes of insects. It would be endless to cite authors and their opinions; especially concerning the bee-tribe. Mr. *Derham* thinks *Swammerdam's* opinion, vide *Swam. Hist. Insect.* p. 92. (who as he was one of the first that rejected equivocal generation, so he was one of the most judicious writers on insects) is the most just, *viz.* that there are three sorts of bees. '1. The king or more properly 'the queen, as being of the female sex. 2. The drones, who 'are properly males. 3. The labouring bees, whose sex we 'cannot distinguish, since we can observe neither male nor fe-'male parts in them, which are very distinct in the drones or 'kings and the queens, by a vulgar error called the kings. 'According to the incomparable anatomist *Joh. Van Horne*, 'there is an *ovarium* in the queens, &c.'

As to what is related by *Aristotle*, *Pliny*, *Virgil*, or any other ancient authors, or by our more numerous modern authors, concerning the production of wasps out of horses; or of bees out of oxen or young bullocks; as also of their polity, their emperors, kings, dukes and common subjects, their exact

disciplin-

discipline and justice, their strict temperance and other virtues, with a great deal more, is so very whimsical, that it is not worth while, to take any farther notice of it: But there is a story told by *Moufet*, in *Theat. Insect. l.* 1. *c.* 8. that deserves our consideration, *viz.* 'that in 1582, being on the highest 'ridges of the *Cartmel-hills* (Mr. *Derham* supposes in *Lancashire*) he observed among the rocks two species of wasps 'desperately fighting; that they differed only in size; that the 'larger trusted to their strength, and the smaller to their numbers; there being six of the smaller ones engaged against only 'one of the larger size; and that the battle was not in the air, 'but among the grass, and lasted for some hours in the hottest 'sun, it not being at an end in two or three hours.' The cause of this engagement according to *Moufet* was, that the large wasps are wont to rob the lesser of their honey and young, or do them some such injury; and the lesser being very revengeful and naturally full of courage, did outbrave even *Mars* himself in assaulting their enemy. But this engagement Mr. *Derham* takes to be such another, as that he has given the account of, namely, one under the conduct of *Venus*, rather than of *Mars*. And as there is no doubt to be made of its being such, and that the engagement seen by *Moufet* was on the highest tops of *Cartmel* (*in summis Cartmeli Montium jugis*) as that Mr. *Derham* saw was on the very top of *Windsor-Chapel*, it may deserve to be considered, whether wasps ever copulate in lower places, obvious to disturbance and every one's observation; or only on such eminencies, where they can be more out of sight; and consequently, in greater safety: And if at any time they should be found in copulation, they may all with safety be seised with the naked hand, provided it can be secured against the queen-wasp, which is the only one in the company, that is provided with a sting.

To conclude these observations about the sexes of wasps, Mr. *Derham* takes notice of *Moufet*'s experiment, (which the former tried) *viz.* if you take a wasp by the feet and suffer her to buz, those wasps, which have no stings, but none of those that have, will fly to her; which some, says he, use as an argument to prove, that some wasps are males, and some females. This experiment Mr. *Derham* was minded to try with a queen-wasp more especially; not knowing, but that wasps, particularly the males, might be as fond of their queens, as the bees are of theirs, who will not forsake them, but live and die with them. But he did not find it succeed so among the wasps: For,

For, tho' he put ſome queen-waſps and others alſo, near the e trance of ſome large waſp-neſts; yet he did not obſerve a flock near them, only now and then one of the common waſ for a little while, to ſee their fellow confined: But, it is tr the queen-waſps he confined, were weak and did not buz lon as alſo the time of copulation was, probably, paſt, it bei *Auguſt* 12. when he tried the experiment.

Two Curious Obſervations on the prodigious ſize of the O mentum *and uncommon Colour of the* Saliva; *by* D John Huxham. Phil. Tranſ. N° 382. p. 60. *Tranſlat from the* Latin.

A Certain woman was for a long time troubled with colic a vomiting, upon which there enſued a hard ſwelling of h belly, that, increaſing daily, grew to a prodigious ſize. A now the patient vomited every thing, black and pure bile, a at length the *fœces* themſelves were thrown up. The pain bei often very ſevere, eſpecially in the left *hypocondrium*, fomen tions, cathartics, glyſters, anodynes and ſeveral other medici were made uſe of; but all to no purpoſe; the pain continu the ſwelling increaſed, and was as hard as a piece of boar From this ſwelling there aroſe ſeveral protuberances, as it we one or two of which were as big as a child's head, and oth as big as a man's fiſt; the largeſt was in the left *hypocondriu* where ſhe felt the moſt exquiſite pain; As the ſwelling creaſed, the patient had a great difficulty of breathing; and af ſhe had ſuffered a deal of pain for almoſt 14 months ſhe length died.

The belly was obſerved to be very protuberant, while the r of the body was exceedingly emaciated. Upon cutting the *domen*, there preſented to view a large maſs of ſuet, as it we (only not ſo white) that filled up the whole cavity of the be in ſuch a manner, that neither the ſtomach, liver or any of t inteſtines, could be ſeen; for, this enormous *omentum* adhe to the *peritonæum* in ſeveral places; eſpecially, in both the *pocondria*, where the adheſion was broader and firmer. Up making a crucial inciſion on the *peritonæum* and ſeparating from the *omentum*, this latter adhered to the whole concave ſ of the liver; the ſtomach was oppreſſed by its weight, as w alſo the *duodenum*, *colon* and *jejunum*; it adhered and was c founded, as it were, with the fatty integument of the kidn eſpecially the left; ſo that the *colon*, a little higher than wh it terminates in the *rectum*, was entirely wrapped up in this c

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creted mass of suet: Hence the passage of the *fæces* was obstructed, and hence proceeded those exquisite pains the patient was tormented with before her death: And to observe this by the bye, for several days before she died, she had no stool either spontaneously or by the sharpest glysters.

Afterwards the *omentum* was separated from the liver, stomach, schirrous pancreas, the intestines, and at length from the mesentery, and the internal *lamina* of the *peritonæum*, that covers the kidneys: The spleen was sunk in some measure into this mass, being thereby contracted and resembling a piece of hide.

This *omentum* weighed 16 *lb.* and ½ averdupois, besides one or two pounds more that adhered to the parts. A prodigious weight! If we consider that the *omentum* of the fattest man scarcely weighs 3 pounds; in a word the Dr. observed none larger, in all the subjects either he himself dissected, or saw dissected by others. It is true there is a much larger *omentum* mentioned by *Gregory Horstius* in his *Obs.* and also in the *Ephemerid. German. Ann.* X: Yet still this may be justly reckoned a prodigious one.

Both in the external and internal substance of this *omentum*, the Dr. observed a great many blood-vessels, exceedingly dilated; some of which were bigger than a goose-quill, and others terminated in aneurisms, as it were: Out of the largest of which he expressed about six ounces of blackish blood, with some whitish *grumæ*; whether these were particles of fat, absorbed by the epiploic veins, and concreted by the distemper, the Dr. could not tell.

This mass seemed to consist of a great many lobes, closely adhering to each other; yet he separated some, of which a few were as big as a small apple, and of much the same shape, the middle was harder than the rest, and could hardly be cut with a knife.

In this subject were also the following remarkable particulars; the lower part of the liver was become schirrous; several *calculi* were taken out of the gall-bladder, of the colour of pit-coal, friable, and floating on water, not improperly called shining bile; for, there were a great many sparkling (undoubtedly saline) particles therein.

The glands of the mesentery were schirrous; nay, some of them almost as hard as a stone; the thin guts were inflamed; the *colon* together with the *cæcum* was almost entirely gangrenous, as also the vermicular process.

Both the kidneys were pretty ſound; the right ſent out two *ureters*, and upon diſſecting the kidney, the Dr. eaſily perceived the reaſon; for, the pelvis of the kidney was divided by a kind of *ſeptum*, a finger thick, entirely of the ſame ſubſtance with the reſt of the *parenchyma* of the kidney; ſo that there was a double kidney, as it were, with a double *pelvis* and a double *ureter*.

There were about two pounds of bloody *ſerum* in the lower part of the *abdomen*.

This diſorder ſeem'd to be entirely incurable, unleſs it could have been prevented at firſt.

Mr. *Fox*, 40 years of age, of a thin, bilious temperament, languiſhed for a month or two with a *nauſea*, jaundice and colic: At length being ſeiz'd with a violent fit of the colic, from drinking plentifully of cyder, he vomited every thing, made but little water, of a lixivious colour, which depoſited much greeniſh ſediment.

The Dr. immediately order'd an emetic potion of *Ipecacuanah*, upon which drinking plentifully of an infuſion of *C. Bened.* he brought up a great deal of viſcous bilious matter; and then by giving him a glyſter of *terebinth.* he had two or three ſtools; upon taking an anodyne mixture, his vomiting and gripes ceas'd; and now the patient complain'd of an extreme langour, and a diſtention of the *abdomen*; and a little after the pain return'd. The Dr. preſcrib'd a *bolus ex jalap.* ℈j. *calomel. gr.* viii. *ſpec. diambr. gr.* vi. *laud. ſolid. gr.* i. *ſyrup. de ſp. cerv. q. ſ.* to be taken with *tinct. ſacr.* ℥ii. Upon this all was quiet again, and 12 hours after, he had three ſtools of a very thin bilious matter: Upon taking an anodyne draught he had a quiet night; in the morning complaining of a pain and ſwelling in his jaws, he ſpit a thick duſkiſh matter; and ſoon after he diſcharg'd a large quantity of a green *ſaliva*, very much reſembling porraceous bile, only thinner. This flux of green *ſaliva* laſted about 40 hours, in which time he diſcharged upwards of two pints; then it inclined to a yellow colour, till at length it became like a ſolution of *gut. gambæ*, the quantity rather increaſing than diminiſhing.

This colour likewiſe continued for 40 hours, and then the *ſaliva* became gradually pellucid; and the ſalivation, as it had begun, ceas'd all of a ſudden.

In two years time he had been troubled twice or thrice with the jaundice, before the acceſſion of this diſorder: Ten years

years before, he had been in great danger of his life by an extraordinary ſalivation, which came on ſpontaneouſly; and then he had not taken the leaſt grain of mercury, nor was his *ſaliva* any ways coloured.

In this caſe the following things are worth obſerving; firſt, a ſpontaneous ſalivation, or probably, raiſed by ſo very ſmall a quantity of *calomel*; for, the Dr. had preſcrib'd ſeveral doſes of the ſame *calomel*, and ſometimes to the quantity of a whole ſcruple, without the leaſt ſign of a ſalivation.

The next thing to be obſerv'd is the uncommon colour of the *ſaliva*; for, the teeth and jaws were tinged green, as if they had been done over with verdigreaſe, and they continued of that colour for 14 days after the patient recovered.

It is farther to be obſerv'd, that this flux was critical, and indicated the jaundice and colic; for, as ſoon as the ſalivation began, the patient had not the leaſt pain in his belly, and the greeniſh colour of his ſkin gradually diſappear'd; he likewiſe made water in greater quantity, but of a blackiſh colour; and tho' before this flux he had been very weak, and almoſt dying, yet he bore well this extraordinary diſcharge of *ſaliva*.

It is well known that the *ſerum* of icteric patients is tinged with the bile; nor need it ſeem more ſurpriſing, that a yellow *ſerum* ſhould be diſcharged thro' the ſalivary glands, than thro' the ſerous veſſels of the *cutis*, as is frequently obſerv'd to be the caſe, after firſt applying a bliſter, not to mention bilious urine. But it is not ſo eaſy to determine whence this green colour of the *ſaliva* ſhould proceed: The Dr. however takes the drinking of the cyder to have been the procatartic cauſe.

A green colour is produced upon mixing an acid with the bile: Hence the green ſtools of children, attended with gripes, by the ſouring of the milk upon their ſtomachs, and hence ariſes a porraceous bile: Now the acid of the cyder being abſorbed by the lacteals, or probably by the meſeraic veins, the conſequence would be a change of the yellow colour of the *ſerum* into a green: He owns, indeed, that acids, nay, that of vitriol, are changed by the *vis vitalis* of the body into an alcalious nature: But when the ſtrength fails, and the concoction is vitiated, and the *lympha* becomes effoete, and the bile unactive, to which if you add the ſlow circulation of the blood, the acids, being but very little alter'd, are diveſted of their acid nature, neither in the firſt paſſages,

 nor

nor in the blood itſelf. This is confirm'd by the acid ſweats of weak perſons, and the acid taſte in the mouths of ſuch as recover from inflammatory fevers, who have had large quantities of acid liquors given them to drink, the Dr. himſelf ſeveral times obſerv'd, that ſoon after drinking plentifully of cyder, he made water that ſmelt ſtrongly of it.

The Dr. waited of a woman, taken with a violent hemorrhage that laſted for ſome time, to whom among other things was given a great deal of the *elix. vitriol. Mynſich.* At length the hemorrhage ceas'd; but the patient, being thereby weakened, fell into ſlight ſpaſms, had pains in her joints, the gripes with a *diarrhœa*; ſhe made ſeveral green ſtools, very like cows dung, for ſeveral days together, tho' ſhe had had three doſes of *rhabarb.* to carry off the acid particles, adhering to the inteſtines: This acid ſalt, mix'd with the blood, was ſeveral days after thrown off into the inteſtines: without undergoing any alteration from the *vis vitalis.*

M. *Leewenhoeck* obſerv'd with his microſcope, that the acid ſalts of the chyle are blunted and comminuted by the bile; but when there is an obſtruction in the liver, acids paſs thro' the lacteals entire: That there was an obſtruction of the bile in the former patient, was evident from his conſtipation, and white ſtools.

The Dr. thinks his hypotheſis may be illuſtrated, if we conſider the cauſe of the green colour of cachectic virgins: For they greatly delight in unripe fruits, vinegar, &c. but the organs of concoction being vitiated, they mix with the blood without undergoing little or no alteration, and overcharge both it and the bile with acids: Hence the glands are obſtructed, the face from being green becomes pale, and hence a dropſical tumour; and what is more, the ſchirrous liver of ſuch as have died of the green ſickneſs, has often been obſerv'd to be green.

Now the Dr. takes the yellow colour of the *ſaliva* ſucceeding the green to be owing to this, *viz.* that after the acid had been partly thrown off from the blood, and partly deſtroy'd by the increaſe of its motion, by the adminiſtering of cardiacs, there only remain'd the ſuperfluous bile to be thrown off, which ſoon happened upon opening the pores of the bile, being in a ſurpriſing manner diſcharged by the wide ſalival ducts.

The

The Effects of Inoculation in New England; *the Eclipse of the Sun in* November 1722; *and the Venom of Spiders; by Dr.* Robie. Phil. Transf. N° 382. p. 67.

THEY had not hitherto obferv'd any ill effects of inoculation in *New England*; but the inoculated were as well, and fome of them a great deal better than ever: As to the bad confequences that have been obferv'd in *England*, Dr. *Robie* could hardly think they were the genuine effects of inoculation, but rather owing to fome previous difpofition to other diftempers, or for want of due evacuations after inoculation, and too foon healing the places of incifion.

As to the great eclipfe of the fun in *November* 1722, the Dr. had an account, that the obfervers at *Yale* College in *Connecticut* Colony fuppos'd it to be but about eight digits: whereas by his calculation for *Cambridge* it was to have been about 11 digits. *Yale* college lies about 8 or 10 minutes weft from *Cambridge*, in about 40° and ½ N. Lat. The Dr. did not calculate it for that place, but for *London*, and it agreed to about 7 or 8′ with Mr. *Derham*'s obfervations, and was pretty exact as to the digits. The gentleman who wrote the Dr. word of this, tells him he calculated it, and could make it fcarce 7 digits eclips'd; and that it went off from *America* into the fea, a great deal more foutherly than the Dr. made it: For by his calculation it was to be centrical at *Cape Cod*, and fo to pafs over to the ifles of *Cape de Verd*: But taking this gentleman to be very much miftaken, the Dr. proceeds to give his obfervations, as far as he was able to make them; and he thinks they were accurately made.

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Nov. 27. 1722.	7	27	in the morning he obferv'd the fun rife eclips'd on its upper *vertex* to the fouth, about four digits; tho' fome on the top of the new college obferv'd it 2 or 3′ before. The fun's true arifing this morning was 7h. 30′: Hence the refraction is about 6′, and fo he often obferv'd it. From this time till about 8h. 30′ or 40′, he faw no more of the fun; but then he judges it was eclips'd fix digits or more.

h. ′ ″

h.	′	″	
8	55	15	The ſun was eclips'd 4 digits and ¾ nearly.
9	00	16	Four digits and ½.
9	19	45	A little ſpot in the ſun emerged.
9	25	45	He obſerv'd the moon go off the ſun.
9	25	45	Mr. *Danforth*, in a room juſt by the Dr. obſerv'd the ſhadow go off the paper about 30° from its lower *vertex* to the eaſt.
9	25	20	Mr. *Appleton* obſerv'd the ſhadow go off the paper, fix'd to the college braſs quadrant at his houſe. Mr. *Owen Harris*, an ingenious ſchoolmaſter in *Boſton*, ſays, he obſerv'd the end at about 26′ after 9.

By the ſecond obſervation the ſun's diameter was to the moon's as 1000 to 972; by the third as 1000 to 975. The eclipſe was obſerv'd at *Boſton*, allowing for its diſtance, as the Dr. obſerv'd it at the college; and at *Barnſtable* on *Cape Cod*, there was but a little left of the ſun; and nearer the head of the cape there was a ring of light quite round the moon.

The teleſcope the Dr. made his obſervations by was 24 foot long; that Mr. *Danforth* made uſe of, thro' which the rays were tranſmitted, was eight foot, and the braſs quadrant the very ſame Dr. *Halley* made uſe of at St. *Helena*. The Dr. took the ſhadow to paſs off *America* about *Cape Cod*, ſuppoſing its Lat. to be 40° N. or 40° 10′, and about 10 or 15′ E. from *Cambridge*.

Sep. 13. 1722. One *Nat. Ware* of *Needham* was bit by a ſmall ſpider (which he could not give an exact deſcription of, having cruſhed it to pieces between his ſtocking and leg) the account he gave the Dr. was as follows, *viz*. That getting up early in the morning, and putting on his ſtocking, he immediately felt ſomething bite his left leg, a little above his ancle; about half an hour after, he felt a pain in that leg, and about half an hour from his firſt perceiving pain in his leg, he felt a pain in his groin, and at the ſame time a creeping pain in the calf of his left leg; and about an hour after, it got into the ſmall of his back, and then round him, and in his ſtomach, and in his right thigh, and afterwards he had a numb-

numbness in his head; the pains were not constant and fixed, but erratic and very acute; his pulse was very low and heavy.

September 14. In the morning the patient came to see the Dr. and was much better, tho' he had but little sleep in the night. The means used were only *Sp. Cor. Cerv.* and *Sal. Vol. Corn. Cerv.* with *Vin. Viperin.* and onions or garlick externally applied to the place, where the wound was. These things raised his pulse, and so, the Dr. supposes, assisted nature to throw off the venom.

Several Observations made in Italy *of a* Lunar Eclipse, *which happened* September 8. 1718. Phil. Trans. N° 382. p. 71. *Translated from the* Latin.

THE following observations were made in the house of S. *Pietro Bembo*, at *Padua*, by S. *Giovanni Poleni* and *Giovambatista Morgagni*.

app. time. *p.* h.	*m.* '	"	
			About the beginning of the eclipse clouds covered the moon.
6	54	25	The shadow comes to the eastern part of *Mare humorum*, is distant from *Aristarchus* the diameter of that *macula* and at the same distance from *Kepler*.
7	5	5	It comes to *Copernicus*.
	12	56	It comes to *Plato*.
	22	31	It entirely covers *Manilius*.
	30	55	It begins to cover the eastern part of *Mare Nectaris*.
	41	53	It comes to the second superior eastern part of *Mare Crisium*.
	46	58	The *penumbra* reaches the extremity of the disk.
	49	4	An almost total immersion.
			All the time of the immersion the moon is observed of a duskish red colour: Just after the immersion, that part of the moon towards the east is more obscured than the rest.
8	33	3	That part of the moon near the middle of the disk is more obscured; and the extreme parts of the disk, quite round, are less obscured.

A little

app. time.

h.	p. ′	m. ″	
9	30	49	A little ſtar, that could not be diſcerned by the naked eye, ſeems to be ſcarce 10 ſeconds diſtant from the moon's diſk, over againſt *Lansbergius*.
	32	9	The *penumbra* becomes clear in the eaſtern extremity of the diſk.
	36	4	The beginning of the emerſion towards the eaſt.
	40	39	Now *Grimaldus* emerges out of the ſhadow.
	44	38	An occultation of the above obſerved little ſtar by the moon, tho' uncertain.
	49	34	*Gaſſendus* emerges.
	50	46	*Mare humorum* entirely emerged.
10	00	3	*Copernicus* emerges.
	5	55	*Plato* begins to emerge.
	14	41	*Eudoxus* emerges.
	19	12	*Menelaus* emerges.
	27	12	*Mare Nectaris* entirely emerged.
	36	28	The ſhadow biſſects *Mare Criſium* by its greater diameter.
	39	12	The ſhadow begins to be ſomewhat rarer.
	41	2	The apparent end of the ſhadow.
	42	57	And that of the *penumbra*.

The following obſervations were made in the palace of the *Iſtituto delle ſcienze*, at *Bologna*; by S. *Geminiano Rondelli*, *Giuſeppe-Antonio Nadio* and *Giulio Cæſare Pariſi*.

true time

h.	p. ′	m. ″	
			The beginning of the eclipſe was not obſerved.
6	51	36	*Mare humorum* at the ſhadow.
	56	22	*Capuanus* at the ſhadow.
	56	37	*Mare humorum* entirely immerged.
7	1	7	*Bullialdus* at the ſhadow.
	2	52	Entirely immerged.
	3	57	*Copernicus* entirely immerged.
	11	22	*Tycho* at the ſhadow.
	12	52	*Tycho* entirely immerged.
	15	37	*Plato* at the ſhadow.

app. time.

h.	p. ′	m ″	
7	16	27	*Plato* entirely immerged.
	19	22	*Manilius* at the ſhadow.
	19	52	*Mare ſerenitatis.*
	23	57	*Mare tranquillitatis.*
	35	8	*Meſſala.*
	36	8	*Meſſala* entirely immerged.
	36	38	*Mare fœcunditatis* at the ſhadow.
	37	23	*Promontorium Somni.*
	39	23	*Cleomedes.*
	39	53	*Mare Criſium.*
	44	8	*Mare fœcunditatis* entirely immerged.
	44	43	*Mare Criſium* entirely immerged.
	47	18	The total obſcuration of the moon, according to S. *Nadio*'s eſtimation.
	47	53	The total obſcuration, according to S. *Pariſi.*
9	33	40	The beginning of the emerſion of the moon.
	36	35	*Grimaldus* entirely emerged.
	40	54	*Galilæus* emerged.
	42	34	A certain conſtellation is covered by the moon, in the ſame vertical nearly with her centre.
	47	50	*Mare humorum* emerged.
	52	10	*Bullialdus* emerged.
	54	25	*Tycho*'s centre emerged.
	55	12	*Tycho* entirely emerged.
	58	46	*Mare Nubium* emerged.
10	4	2	*Plato* at the termination of the ſhadow.
	5	33	*Plato* entirely emerged.
	17	12	*Inſula ſinus medii* emerged.
	23	47	*Meſſala* at the termination of the ſhadow; and at the ſame time *Mare ſerenitatis* entirely emerged.
	27	58	*Mare tranquillitatis* emerged.
	30	12	*Cleomedes* emerged.
	32	8	*Mare Criſium* touches the extremity of the ſhadow.
	34	7	*Mare fœcunditatis* entirely emerged.
	36	19	*Mare Criſium* entirely emerged.
	37	39	The end of the eclipſe.

The following observations were made in the suburbs of *Bologna*, southwards by *S. Eustachio* and *Gabrielo Manfredi*.

app. time.			
	p.	*m.*	
h.	′	″	
6	31	48	Now the moon begins to rise out of the hills, tinged with the *penumbra* of the atmosphere.
	42	13	The beginning of the true eclipse, as far as could be judged by the uncertain termination of the shadow.
			A little after the moon was covered by clouds and intercepted by trees.
	52	48	The shadow extends over *Aristarchus* and *Kepler*, and at the same time seems to touch *Mare humorum*.
7	2	23	The shadow passes thro' the middle of *Bullialdus*, and at the same time touches *Copernicus*.
	4	2	The shadow passes thro' the middle of *Copernicus*.
	5	4	*Copernicus* entirely covered.
	7	5	The shadow reaches *Pitatus*.
	10	54	——————— *Tycho*.
	12	19	The middle of *Tycho* covered.
	13	9	*Tycho* entirely covered.
	15	34	The shadow at *Plato*.
	16	7	At the middle of *Plato*.
	16	54	*Plato* entirely covered.
	20	9	*Manilius* covered.
	20	34	The shadow touches *Mare serenitatis*.
	23	44	*Menelaus* covered.
	24	36	*Dionysius* covered.
	27	34	*Plinius* covered.
	29	49	The shadow at *Catharina*, *Theophilus*, and *Cyrillus*.
	30	36	The shadow touches *Fracastorius*.
	31	44	The middle of *Fracastorius* covered.
	32	34	*Promontorium acutum* immerged.
	35	15	*Promontorium Somni*.

app. time.			
	p.	*m.*	
h.	′	″	
	37	57	*Taruntius.*
	39	39	The ſhadow touches *Mare criſium.*
	42	16	It paſſes thro' the middle of *Mare criſium.*
	44	5	*Mare criſium* entirely immerged.
	47	50	The total immerſion of the moon.
			All the time of the eclipſe the moon was very diſtinctly ſeen in a clear ſky, and of a reddiſh colour, denſer, where it was deepeſt immerged into the ſhadow.
8	38	50	At this time, and for ſome minutes after, the face of the moon appeared equally obſcured on all hands: Whence it was plain, the eclipſe was nearly central.
9	27	50	Over againſt *Grimaldus*, where the emerſion was to happen, there began to appear a remarkable brightneſs.
	29	20	The beginning of the emerſion doubtful.
	33	20	The emerſion undoubtedly began.
	35	21	*Grimaldus* began to emerge.
	35	55	The center of *Grimaldus* emerges, and now *Riccioli* entirely emerged.
	36	26	*Grimaldus* entirely emerged.
	39	28	*Galilæus* emerged.
	41	22	The ſhadow touches *Mare humorum.*
	42	31	The little ſtar, which continued for ſome time near the inferior limb of the moon, was now at length hid by the moon, about *Tycho*, that was ſtill eclips'd: Other little ſtars about to be hid under the moon; but after that one or two digits of the moon's diſk began to ſhine, all vaniſh'd by reaſon of its brightneſs.
	43	53	The ſhadow paſs'd thro' the middle of *Mare humorum.*
	45	41	*Ariſtarchus* emerges.
	47	23	*Keplerus* emerges.
	52	6	*Bullialdus* emerges.
	53	31	*Tycho* begins to emerge.
	54	9	The middle of *Tycho* emerges.

app. time.			
h.	*p.* ′	*m.* ″	
	55	1	*Tycho* entirely emerged; at which time the little star had not yet emerged out of the moon.
	55	21	*Copernicus* begins to emerge.
	56	6	The middle of *Copernicus* emerges.
	59	15	*Copernicus* entirely emerged.
10	00	1	The little star which a little before was hid under the moon, was now observ'd at some distance from her limb, and seem'd as if it had emerged four or five minutes before; it was over against the obscure part of the moon's lower limb, not far from the termination of the shadow.
10	4	51	The shadow pass'd thro' the middle of *Plato*.
	5	36	*Plato* entirely emerged.
	13	6	*Manilius* emerges.
	16	31	*Dionysius* emerges.
	16	41	*Menelaus* emerges.
	20	31	*Fracastorius* entirely emerged.
	23	51	*Snellius* and *Furnerius* entirely emerged.
	24	5	*Promontorium acutum* emerged.
	25	11	*Messala* entirely emerged.
	31	11	*Proclus* emerged.
	31	51	*Mare crisium* begins to emerge.
	34	3	The middle of *Mare crisium* begins to emerge.
	36	7	*Mare crisium* entirely emerged.
	38	5	About this time the true shadow seemed to have gone off the moon's disk, while the *penumbra* continued thereon a considerable time.

In the *Ephemerides*, publish'd in 1715, from M. *Cassini's* tables, for the use of the *Istituto Bolognese delle Scienze*, the beginning of these eclipses was mark'd at 6h. 41′: The total immersion at 7h. 46′, the begining of the emersion at 9h. 33′; the end at 10h. 38′, which times scarcely differ one or two minutes from the times observ'd.

The following observations were made by the Marquis *Antonio Ghisilieri*, on the observatory in his house at *Bologna*.

app.

app. time.	p.	m.	
h.	′	″	
6	40	23	The beginning of the eclipſe doubtful.
	51	23	*Mare humorum* at the ſhadow.
	55	46	*Capuanus* at the ſhadow.
7	1	13	*Bullialdus* at the ſhadow.
	28	14	*Mare nectaris* entirely immerged.
	32	30	*Promontorium acutum* at the ſhadow.
	36	45	*Promontorium ſomni* at the ſhadow.
	38	45	*Mare criſium* at the ſhadow.
	46	37	The total immerſion of the moon.
9	33	50	The beginning of the emerſion.
	35	39	*Grimaldus* entirely emerged.
	54	17	*Tycho* entirely emerged.
10	15	6	*Plinius* entirely emerged.
	32	59	*Mare criſium* begins to emerge.
	37	42	The end of the eclipſe.

Experiments and Obſervations on the freezing of Water *in* vacuo; *by M.* Fahrenheit. Phil. Tranſ. N° 382, p. 78. *Tranſlated from the* Latin.

AMONG the ſeveral ſurpriſing phænomena of nature M. *Fahrenheit* always thought the freezing of water not the leaſt conſiderable: Hence he was often deſirous of trying what would be the effects of cold, upon including water in *vacuo*: And becauſe the ſecond, third, and fourth of *March* 1721 O. S. were favourable for that purpoſe, he made the following experiments and obſervations.

Before he comes to the experiments themſelves, he premiſes ſome things on his thermometers, the diviſion of the ſcale, and his method of exhauſting them. His thermometers were chiefly of two ſorts; one fill'd with ſpirits of wine, the other with mercury; and their lengths were different, according to the uſes they were deſign'd for; but they all agreed in having the ſame number of degrees, and in the fixed limits of their variations. The ſcale of thermometers for meteorological obſervations only, begins below at 0 and ends at 96 degrees. There are three fix'd limits in this diviſion; the firſt is in the loweſt part or beginning of the ſcale, and produced by the commixture of ice, water and ſal-armoniac or even ſea ſalt; if into this mixture you put the thermometer, it deſcends to 0; this experiment ſucceeds better in winter than in ſummer: The ſecond

ſecond limit is when water and ice are mix'd together without the above-mentioned ſalts; for, putting the thermometer into this mixture, it is at 32 degrees, which M. *Fahrenheit* calls the limit of initial congelation: For, ſtagnant waters are cover'd with a thin cruſt of ice in winter, when the thermometer is at that degree: The third limit is at the 96th degree, to which the ſpirits are dilated, while the thermometer is held in the mouth or under the arm-pits of a healthful perſon, till it perfectly have acquir'd the ſame degree of heat with the body. But if we want to find the heat of a man in a fever or any other hot diſtemper, we muſt make uſe of another thermometer, whoſe ſcale extends to 128 or 132 degrees: But whether theſe degrees are ſufficient for finding the greateſt degree of heat of any fever, M. *Fahrenheit* had not hitherto experienced; yet it is ſcarce credible, that the degree of heat of any fever ſhould exceed the above-mentioned number of degrees. The ſcale of the thermometers for determining the degrees of heat in boiling liquors, does alſo begin at 0 and contains 600 degrees; for, the mercury itſelf (with which the thermometer is fill'd) begins to boil at about the ſame degree.

That the thermometers may be the ſooner affected with all the changes of heat, they are provided with glaſs cylinders inſtead of balls: For, this, by reaſon of the greater quantity of ſuperfices, they are ſooner penetrated by the different degrees of heat.

M. *Fahrenheit*'s method of exhauſting was thus; a glaſs ball (as repreſented Fig. 6. Plate XI.) furniſhed with a ſmall tube B C three or four inches long, and tapering at the extremity C, is heated over a fire; after which the extremity of the tube is immerſed in water, till by the cooling of the air, contain'd in the ball, it become fill'd with ſome drops of the water; and then it is held with a pair of tongs over the broader flame of a lamp, or over live coals, till the water in the ball begin to boil, and the vapour forcibly burſt out: This boiling is continued for ſome time, after which the ball is remov'd from the fire, and the flame of a candle applied to its extremity. As the ball cools, the vapour rarified by the fire, is ſucceſſively condens'd, and its egreſs gradually diminiſh'd; and as ſoon as it entirely ceaſes, the extremity of the tube melts, and the ball is hermetically ſeal'd and exhauſted. Whether all the air contain'd therein be well exhauſted in this manner may be tried, by breaking the extremity of the tube

under

under mercury; for, then the ball will be entirely fill'd with mercury, if it be carefully broken without admitting the external air: You may alſo break it under water; but tho' it be done never ſo carefully, the ball will not be compleatly fill'd with water: For, while the water enters the exhauſted ball, the air which is always mix'd with water, ſeparates from it into very ſmall bubbles, which after uniting, form a larger one in the ball: In the ſame manner the ball may be exhauſted, if the third, the half, or a greater part of the ball be requir'd to be fill'd with water: For, it is firſt fill'd with the deſired quantity of water, and then, after boiling the water, it is hermetically ſeal'd.

The experiments were as follows; *March* 2. 1721. he expos'd to the cold a glaſs ball, about an inch in diameter, exhauſted in the manner above-mentioned; and fill'd with rain-water almoſt half full: The temperament of the air was in the thermometer mark'd at 15 degrees. In an hour after, he found the water ſtill fluid in the ball, the reaſon of which he took to be, that the cold had not thoroughly penetrated it: In order to remove all doubt, he left the ball, expos'd all night in the open air. Next day, *viz.* the third of *March* at 5 o'clock in the morning he found the water ſtill fluid, and the thermometer at the ſame degree; the cauſe of which unexpected phænomenon he attributed to the abſence of the air. In order to diſcover the truth of this conjecture he broke the extremity of the tube, that the exhauſted ball might be again fill'd with air; upon which, the whole maſs of water was all of a ſudden mix'd with very thin *lamellæ* of ice. Before he repeated the experiment, he would try by another, whether theſe icy *lamellæ* would float in the water; wherefore he broke the ball, and putting ſome of the ice into ſome water in a glaſs cup, he obſerv'd it float therein.

Viewing the cup again, he in a little time obſerv'd all the water mixed with icy *lamellæ*; yet the greateſt part of the water ſtill continued fluid between the interſtices: The thermometer, put into this mixture, ſtood at 32 degrees. M. *Fahrenheit*, being deſirous to view this phænomenon more attentively, reſolv'd to repeat the experiment with two other balls, and after preparing them in the manner above-mentioned, he expos'd them for an hour in the open air, the thermometer was then at 20 degrees: An hour after, he found the water ſtill fluid in both the balls; but after that the exhauſted ball was again fill'd with air, the water (as in the former experiment)

was

was very ſoon mixed with icy *lamellæ*; and their production was ſo inſtantaneous, that it could hardly be obſerv'd with the eye: And becauſe the production of the *lamellæ*, in the glaſs cup, had eſcap'd his obſervation, he was ſtill very deſirous to view that phænomenon with ſomewhat more attention. Before he broke one of the balls, he ſeparated the water in the ſaid cup from the icy *lamellæ*; upon which he broke the ball, and threw the ice into water; the ice, it is true, floated on the water, but he in vain expected the production of the *lamellæ* in the cup: M. *Fahrenheit* poſtpon'd the further trial of theſe experiments till 11 o'clock at night, when he expos'd three balls in a keen froſt: Two of theſe balls were again fill'd with water about half full; and the third ball was fill'd about ¾ parts full: The temperament of the air was by the thermometer mark'd at 26 degrees; at 4 o'clock in the morning he found the temperament of the air the ſame by the thermometer, and the water in the two balls, which were only fill'd half full, ſtill fluid; but in the third ball, the water was frozen and the ball broken: The ice was intermixed with very ſmall bubbles, but few in number, and its tranſparency was very much interrupted, and reſembled the confus'd cryſtallization of ſome ſalts. M. *Fahrenheit* attributed this to ſome imperceptible fiſſure, by which the external air had inſinuated, and ſo produced the congelation of the water.

M. *Fahrenheit*, being ſtill very deſirous of viewing the production of the *lamellæ* in the glaſs cup, took the ſaid veſſel into the room where the former experiments were made, and happening to ſtumble a little, the water in the glaſs was agitated, and in that very inſtant the whole maſs appear'd intermixed with ſeveral icy *lamellæ*: By this accident he diſcover'd, that ice could be produced in pretty cold water by agitating it; and this put him on trying whether water would freeze in *vacuo* by agitation. Therefore, after ſhaking the ball a little, he had the pleaſute of obſerving the ſame phænomenon, and at the ſame time found his miſtake in attributing the fluidity of the water to the abſence of the air. In the mean time he found by the thermometer, that the froſt was much abated; for, it now ſtood at 28 degrees, and therefore he ſoon diſſolv'd the ice with his hand, and again expos'd one of the balls in the open air, where leaving it about half an hour, he obſerv'd the froſt remit ſtill more; for now the thermometer ſtood at 32 degrees; and becauſe he

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was afraid, that by the remitting of the cold, it would be to no purpose to repeat the experiment, tho' the ball were left exposed longer in the open air; he at that very time attempted to produce a congelation in the water by shaking the ball; but tho' he shaked it strongly, not the least signs of it appeared. Having no hopes of producing it in this manner, he still had a mind to try, whether it would succeed, if the exhausted ball were again filled with air. Breaking therefore the extremity of the tube, very minute *spicula* of ice were diffused thro' the whole mass of water, which, by shaking it round, mounted to the surface, and afforded a very agreeable sight by the reflection of the light from their smooth superficies.

Of the Difference in the height of a Human Body, *between Morning and Night; by Mr.* Wasse. Phil. Trans. N° 383. p. 87.

MR. *Wasse*, having measured a great many sedentary people and day-labourers of all ages and shapes, found the difference of their height in the morning and at night to be near an inch. He tried himself, when sitting, and found it in the like manner; particularly, *August* 21. 1728, weather warm, no wind, he sat down at 11 o'clock in the morning, and fixed an iron-pin, so as to touch it, and that but barely; afterwards he fatigued himself for $\frac{1}{2}$ an hour with a garden-roller; and the consequences was, that at 12^h $30'$ he could not reach the nail sitting, by about $\frac{5}{10}$ of an inch: At 2 o'clock the same day he wanted near $\frac{6}{10}$ of an inch. On the 21st at 6^h $30'$ in the morning, he touched the nail fully; and after the abovementioned exercise for only $\frac{1}{4}$ of an hour, at 7^h $14'$ he fell short, almost as much as before. On the 27. having sat up late, he was faint and felt himself heavy upon the ground, and without any spring, and at 9 that morning he did not reach the nail, tho' he had used no exercise; he rid out, but he could not reach it that day. On the 28. he rid about 4 miles; and whereas at 6 that morning he reached the nail, by 8 he had lost $\frac{6}{10}$ of an inch. *September* 19. he came somewhat tired from *Oxford*, and next morning at 8 he wanted $\frac{1}{2}$ an inch. If he studied closely, tho' he never stirr'd from his writing desk; yet in five or six hours he would lose near an inch. All the difference he found between labourers and sedentary people is, that the former are longer in losing their morning height, and sink rather less in the whole than the

the latter. He could not perceive, that when the height i loſt, it can be regained by any reſt that day, or by the uſe o the cold bath. *April* 30. 1724. Mr. *Waſſe* meaſured his mar (who was 7 years old, ſtrong, ſhort back'd and well legged before and after riding 20 miles, and could not perceive th leaſt difference in her height. The alteration in the huma ſtature, he ſuppoſes, proceeds from the yielding of the car tilages between the *vertebræ* to the weight of the body in a erect poſture.

This curious obſervation was confirmed by ſeveral member of the *Royal Society*, and others who purpoſely tried the ex periment.

Remarks on the foregoing Obſervation; by Mr. Becke Phil. Tranſ. N° 383. p. 89.

MR. *Becket* obſerved farther, that the alteration has bee more conſiderable in young perſons than in thoſe that hav been aged. The trials, equally ſucceeding in a ſitting as in ſtanding poſture, will naturally lead us to believe, that it mu neceſſarily be from the trunk of the body, or ſome of its part that this remarkable alteration is brought about. Now ever body knows, that the ſtandard of a perſon's ſtature has alway been looked upon to be determined by the whole *compages* o the bones, adjuſted by the divine architect, according to th ſtricteſt rules of geometry: But there is ſomething ſo ſurpriſin in the ſtructure and diſpoſition of the ſpine, that nothing bu ſuch a peculiar contrivance could ſo curiouſly have fitted it fo the reſpective uſes and purpoſes it was ordained for. Th thickneſs and ſhortneſs of the bones, with the intervening ca tilages, aſſiſted by the bony proceſſes, diſpoſe it to a motion pe culiar to itſelf: Whereas, had the bones been of any conſider able length, upon bending the body, the articulations muſt hav formed a large angle upon their innermoſt edges, and the ſpina marrow continually liable to be injured; or had the cartilage been entirely wanting, it would have been as uſeleſs, as if i were but one bone; whereby being rendered incapable of bend ing the trunk of the body, it muſt have always remained in a erect poſture. But by the preſent diſpoſition of its parts, it i not only abſolutely ſecured againſt any ſuch inconveniences; bu ſo ſmall a pillar is capable of ſupporting without hazard ſuch prodigious weights, as we are not wanting in our accounts of Another particular which beſpeaks the utmoſt wiſdom and de ſign in the contrivance of this part is the remarkable difference in

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the thickneſs of the cartilages placed between the bones of the ſpine; the *vertebræ* of the breaſt requiring but little motion, the cartilages there are but thin, in compariſon of thoſe of the loins, which being very thick, the loweſt more eſpecially, the motion is there vaſtly greater; and the cartilages being abundantly thicker before than behind, this is the reaſon that we bend our bodies ſo much more forwards than backwards: And by this admirable method of diſpoſing the thicker parts of the cartilages forwards, it is, that in all violent exerciſes, the parts contained in the belly and breaſt are in a great meaſure ſecured from any damages to which they might have been liable; becauſe by the pliableneſs and elaſticity of theſe cartilages, they break the violent ſhocks the *viſcera* muſt otherwiſe have neceſſarily ſuſtained upon ſuch occaſions. From what Mr. *Becket* has here remarked, in relation to theſe peculiar properties of the cartilages, placed between the bones of the ſpine, we may reaſonably ſuppoſe them to be certain compreſſible, dilatable, elaſtic bodies, which, like other bodies, endued with the ſame qualities, will naturally yield to any incumbent weight, ſufficient to force the particles of matter of which they conſiſt, into a more cloſe union; and that when this compreſſive force is removed, they will of themſelves recover their former ſtate: Now it is particularly to be obſerved, that the loweſt of all the cartilages of the loins is the thickeſt; and ſo conſequently, that it contains a greater quantity of matter than any of the reſt: By which means it becomes more diſpoſed to have its thickneſs diminiſhed, and that all of them gradually become thinner, even to the top of the ſpine. Now all ſuperior bodies if they come to an immediate contact, preſſing upon their inferior, it muſt neceſſarily follow, that the whole weight of the body, excepting the lower limbs, muſt preſs upon and be ſuſtained by the loweſt *vertebræ* and their cartilages: But theſe cartilages, as has been obſerved, being much thicker in this part than the other, and the incumbent weight bearing harder upon them, they muſt be unavoidably compreſſed more than the other; and ſo conſequently, when this weight is removed, their expanſion, by reaſon of their natural elaſticity, will alſo be greater. This being the natural ſtate and diſpoſition of theſe parts, during the whole ſpace of time we are uſually employed about our neceſſary avocations, till we diſpoſe ourſelves to reſt, the cartilages of the ſpine will, by their compreſſible and yielding properties, become more cloſe and compact from the perpendicular preſſure they ſuſtain; and ſo conſequently the ſpine, the only ſupport of

the trunk of the body, will become shorter. But when this superior weight shall be entirely removed, by placing the body in an horizontal posture, as it always is, when we are in bed, the compressed cartilages will, by their natural elastic power, begin gradually to enlarge themselves, till they recover the same expanded state they enjoyed, before they were forced to yield to the incumbent pressure; and so consequently, it will produce a considerable alteration in a person's stature, agreeable to the determined times mentioned in the preceeding *Transaction*. For, if we only consider, that the aforesaid compressive force will lessen the thickness of all the cartilages, in proportion to the quantity of matter they contain; and that they are usually about 24 in number, it will be no difficult matter to apprehend, that their natural expansion being recovered by rest, the aggregate of the whole of the expansions may amount to about an inch. Now if the alteration be so considerable, occasioned only by the bare incumbent weight of the superior parts of the body, without any additional force applied to compress the cartilages still closer; how much more may we reasonably imagine it would be, were the experiment tried on such persons, whose usual employment it is to carry heavy burdens. Mr. *Becket* only observes this one particular, which is, that this alteration is not to be expected to be the same in aged persons, as in those that are younger; because, the cartilages, as we advance in years, gradually grow harder, till many of them arrive to the solidity of a bone, that is, by degrees they lose their spring or expansive power, and at length continue in a compressed state of rest. And this is, undoubtedly, one principal cause, why old people not only seem to have lost somewhat of their height, but are actually shorter.

The Variation of the horizontal Needle *at* London *in the latter end of the Year* 1722 *and the beginning of* 1723; *by Mr.* George Graham. Phil. Transf. N° 383. p. *96*.

THE figure of the three needles, with which the experiments were made, was prismatic; their length 12,2 inches nearly; their extremities, which pointed to the divisions being filed to an edge, which made a fine line, perpendicular to the horizon. The caps of two were of crystal, the other of glass; they were well polished on the inside, in that part which touched the pin they moved upon. The box was brass, and of a breadth sufficient to admit 20 degrees on each side the middle line, and covered with a piece of ground glass; the circular

arches

arches at the ends were raiſed ſo much above the bottom of the box, as to have their upper ſurfaces, upon which the diviſions were cut, lie in the ſame plane with the needle, and at ſuch a diſtance from each other, that the needle might play freely between them. A few of the degrees at the north end were divided into ſix equal parts, each diviſion being 10 minutes. It was eaſy, by the help of a convex glaſs, to determine the pointing of the needle to leſs than a quarter of theſe diviſions, or to about two minutes of a degree. The pin, upon which the needle moved, was of ſteel hardened and ground to a fine point; and by a ſpring placed in the box, the needle might be raiſed from off the point, and let down again at pleaſure, without removing the glaſs, or diſturbing the box. By this means both the ſharpneſs of the point and poliſh of the cap, were better preſerved from injury, when there was occaſion to move the box. A ſmall piece of braſs was made to ſlide upon that end of the needle, which pointed to the ſouth, for readily bringing it to an horizontal poſition; for, according to the different ſtrength of the touch, the north end of the needle will dip more or leſs. The bottom plate of braſs was a little broader and longer than the box; and its edges made lines, exactly parallel to the middle line of the diviſions; and for the greater ſecurity of placing the box in a right ſituation, there was a braſs ruler 30 inches long, having its edges even and parallel, excepting part of that edge, which was applied to the ſide of the box, that was a little filed off on the middle, that the ſide of the box, near its ends only, might touch the ruler. By this contrivance the two points of contact were as far aſunder, as the length of the box would admit of, and the other edge of the ruler making a longer line than the ſide of the box, afforded a better direction for giving it the ſame ſituation.

For determining the quantity of the variation, Mr. *Graham* got a meridian line, ſtretched upon the top of the houſe, between the rails of the leads, which were upwards of 15 foot aſunder, and the line was a little more than 39 inches above the leads. As this line was faſtened to two pieces of braſs, that were fixed in the rails, and was upwards of 15 foot long, no ſenſible error could ariſe in putting it up at any time. The compaſs box was placed upon a wooden ſtool, with three feet, that had nothing of iron about it, and its top ſet level by a plumb-rule: But finding, that in the open air the wind gave ſome diſturbance, he put up another line, after the ſame manner, in a room two pair of ſtairs high; this line was about the ſame

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the trunk of the body, will become ſhorter. But when this ſuperior weight ſhall be entirely removed, by placing the body in an horizontal poſture, as it always is, when we are in bed, the compreſſed cartilages will, by their natural elaſtic power, begin gradually to enlarge themſelves, till they recover the ſame expanded ſtate they enjoyed, before they were forced to yield to the incumbent preſſure; and ſo conſequently, it will produce a conſiderable alteration in a perſon's ſtature, agreeable to the determined times mentioned in the preceeding *Tranſaction*. For, if we only conſider, that the aforeſaid compreſſive force will leſſen the thickneſs of all the cartilages, in proportion to the quantity of matter they contain; and that they are uſually about 24 in number, it will be no difficult matter to apprehend, that their natural expanſion being recovered by reſt, the aggregate of the whole of the expanſions may amount to about an inch. Now if the alteration be ſo conſiderable, occaſioned only by the bare incumbent weight of the ſuperior parts of the body, without any additional force applied to compreſs the cartilages ſtill cloſer; how much more may we reaſonably imagine it would be, were the experiment tried on ſuch perſons, whoſe uſual employment it is to carry heavy burdens. Mr. *Becket* only obſerves this one particular, which is, that this alteration is not to be expected to be the ſame in aged perſons, as in thoſe that are younger; becauſe, the cartilages, as we advance in years, gradually grow harder, till many of them arrive to the ſolidity of a bone, that is, by degrees they loſe their ſpring or expanſive power, and at length continue in a compreſſed ſtate of reſt. And this is, undoubtedly, one principal cauſe, why old people not only ſeem to have loſt ſomewhat of their height, but are actually ſhorter.

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THE figure of the three needles, with which the experiments were made, was priſmatic; their length 12,2 inches nearly; their extremities, which pointed to the diviſions, being filed to an edge, which made a fine line, perpendicular to the horizon. The caps of two were of cryſtal, the other of glaſs; they were well poliſhed on the inſide, in that part which touched the pin they moved upon. The box was braſs, and of a breadth ſufficient to admit 20 degrees on each ſide the middle line, and covered with a piece of ground glaſs; the circular arches

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length with the other, and 39 inches above the floor. Some time after he put up a third line, of the ſame length, in the room over this. By the method made uſe of in fixing theſe lines, they could not differ above 2′ of a degree from the meridian, or from each other. Before he had made any trials, he imagined no other difference would ariſe than what might be occaſioned by the friction of the needle upon the point it was to move on, and having found that conſiderable in all the needles he had taken notice of, he took more than ordinary care to provide againſt it, and ſucceeded beyond his expectation. For, he ſeveral times obſerved all the three needles return ſo exactly to the ſame place, that he could not perceive the leaſt difference; as likewiſe all three agree very nearly about the ſame time, when they have been placed in the ſame box immediately one after another, the box remaining unmoved. The firſt needle he made, was a little above three tenths of an inch broad, about ,06 in thickneſs, and weighed about an ounce *Troy*, together with the cap of cryſtal. After ſome trials with this needle, it was made narrower, not to exceed half a tenth of an inch, and it then weighed 5 penny weight and 5 grains. The ſecond needle was at firſt about 3 tenths of an inch broad, and ,04 thick, together with the cap of glaſs; and after ſeveral trials it was made ſo much narrower, that its breadth was ſomewhat leſs than its thickneſs, and it weighed 2 penny weight and 5 grains. The third was nearly of the ſame dimenſions with the ſecond and weighed 2 penny weight and 3 grains. When the two firſt needles were made narrower, care was taken, that the files, made uſe of for filing the north ends, did not touch the ſouth ends; and after they were made lighter, he tried them both, before they were freſh touched upon the ſtone, and found no ſenſible difference in their direction. The reaſon of making the two firſt needles ſo heavy was to try, whether they would return more conſtantly to the ſame ſituation than lighter ones. But notwithſtanding each of them would ſettle very exactly in the ſame place, for a great number of trials made immediately after each other; yet he found them at different times to differ conſiderably from their former directions.

This occaſioned his making them narrower, fearing their breadth had ſome way contributed to their irregularity. But after the alteration, he found the ſame thing happened, tho' he could find nothing of it to proceed from any friction upon the point. This made him prefer the lighter needles, as leſs apt to injure the point they moved upon, and as exact in returning to the

the ſame ſituation. After ſeveral trials, he found all the needles he made uſe of, would not only vary in their direction on different days; but frequently at different times of the ſame day; and this difference would ſometimes amount to upwards of ½ a degree in the ſame day, ſometimes in a few hours. And this alteration he obſerved, whether the needles were drawn aſide immediately before the obſervation, or ſuffered to remain undiſturbed. For, he has left the box ſtanding for ſeveral days together, without ever diſturbing the needle, only he has taken notice what it pointed at, and the time of the day, and he could ſometimes perceive a very ſenſible alteration in a few minutes. But whether it ſtood near its greateſt or leaſt variation, or whether he drew the needle to one ſide with a key a few degrees or a greater number, it would conſtantly return to the ſame place it ſtood at immediately before. Sometimes he took the needle out of the box and put it in again, and this he ſeveral times repeated in an hour. At other times he took down the box from off the ſtool, and put it up again; but found no alteration in its direction: So that he found it of no conſequence, whether the needle was drawn aſide or let alone, the ſhaking of the floor by walking upon it, or the trembling of the houſe by the coaches in the ſtreet, was ſufficient to overcome the ſmall friction upon the point. When Mr. *Graham* made the obſervations, he was very careful to have no keys nor iron about him, that could affect the needle.

The box was placed in the room upwards of ſix foot from the neareſt wall, and upwards of 13 foot from the grate in the chimney, and no iron could at any time be brought near it without his knowledge. Yet after all, he was not ſatisfied, that it was out of the reach of iron, and that the variation ſhewn by it is the true quantity; but he was very ſure there was no change of circumſtances in the room, that could affect it; for, if there were any ſuch materials in the wall, or floor, their diſtances and ſituations continued the ſame. But for a farther confirmation of this irregularity, he put one of the needles into a wooden box, divided with a few degrees, as the other, and placed it at the ſame meridian line, at the diſtance of three foot and ½ from the other, and found both needles nearly agreed in their alterations. The needles were all touched by that excellent loadſtone, preſented to the *Royal Society* by the Lord *Paiſley*. It is to be taken notice, that the needles were not touched upon the naked ſtone, but with its armour on, generally upon that part of the capping neareſt the poles; but he could find no difference in the

the direction, by touching upon another part: Farther, when he observed the needle increasing or decreasing in its variation, he has very frequently, with a key, drawn it the contrary way several degrees, and then letting it return very gently, till it has been within a degree or less of the place it stood at immediately before, he stopped it there for some time, by holding the key at a proper distance; and withdrawing his hand gradually, tried to make it stand short of its former place, but could never succeed. By this and several other methods made use of, he was well assured these changes in the direction are owing to some other cause than the friction of the needle upon the pin; but what that cause is he could not tell: For, it seems to depend, neither on heat or cold, a dry or moist air, clear or cloudy, windy or calm weather; nor on the height of the barometer. The only thing that has any appearance of regularity is, that the variation has been generally greatest, for the same day, between the hours of 12 and 4 in the afternoon; and the least about 6 or 7 in the evening.

March 8. 1722. A piece of brass was fixed to a wooden box; and a few degrees were divided into 10′ each, as in the brass box, to try, if both needles would be alike affected in the several alterations. This wooden box was placed at the same meridian line, and about the distance of three foot and ½ from the other.

	Brass box. Needle 2 = 5. ° ′	h. ′	Needle 2 = 5 ° ′
March 8.	14 = 30 —	3 = 00	14 = 25 +
	14 = 20	3 = 15	14 = 20
	14 = 15 +	4 = 00	14 = 10
	14 = 20	4 = 15	14 = 15
	14 = 25	5 = 00	14 = 20
	14 = 25	5 = 30	14 = 20
	14 = 15	5 = 45	14 = 10
	14 = 00	5 = 57	14 —
	14 —	6 = 8	13 = 55
	13 = 50	6 = 15	13 = 40
	14 = 20	6 = 38	14 = 15 +
	14 +	6 = 48	14 = 00
	14 = 00	6 = 54	14 —
	14 = 5	7 = 5	14 +
	14 = 10	7 = 15	14 = 5
	14 +	12 = 00	14 +

Brass

Brass box:	Needle 2 = 5.		Needle 5 = 5.
March 9.	° '	h. '	° '
	14 = 10	9 = 30	14 = 10
	14 = 10 +	10 = 00	14 = 10 +
	14 = 10	10 = 15	14 = 10
	14 = 10 +	10 = 30	14 = 10 +
	14 = 15	11 = 00	14 = 15 —
	14 = 00	8 = 15	14 = 00
	14 = 00	11 = 50	14 = 00
March 10.	° '	h. '	° '
	14 = 10 +	10 = 00	14 = 10
	14 = 15	11 = 00	14 = 10 +
	14 = 15	12 = 00	14 = 10 +
	14 = 15 +	12 = 45	14 = 10 +
	14 = 15 +	1 = 00	14 = 10 +
	14 = 15 +	1 = 30	14 = 10 +
	14 = 15 +	1 = 45	14 = 10 +
	14 = 15 +	2 = 00	14 = 10 +
	14 = 15	3 = 30	14 = 10
	14 = 15 +	4 = 00	14 = 10 +
	14 = 15 —	5 = 30	14 = 10 —
	14 = 10	6 = 00	14 = 5
	14 = 00	6 = 15	14 = 00
	14 —	6 = 30	14 —
	14 +	7 = 30	14 +
	14 = 5	7 = 45	14 +
	14 +	12 = 00	14 +

March 30.

The needle 2 = 5 which was in the braſs box, was this day put into the wooden box, and a new needle put into the braſs box, weight 2 = 3.

No remarkable change happened to either needle till *April* the 5.

Needle

	Needle 2 = 3 in the brass box.		Needle 2 = 5.
	° ′	h. ′	° ′
April 5.	14 = 5	9 = 00	14 + 00
	14 = 10	1 = 30	14 = 5
	14 = 10 —	5 = 30	14 +
	14 —	8 = 15	14 —
	13 = 50 +	8 = 37	13 = 45
	13 = 55 +	9 = 45	13 = 45 +
	14 —	10 = 25	13 = 50
	14 = 00	10 = 45	13 = 55
	14 +	11 = 00	14 = 00

The first column shews the variation of the needle in the brass box. The third the variation of that in the wooden box. The second column shews the time, by the clock, when the observations were made.

April 15. 1723. ° ′	h. ′	April 16. ° ′	h. ′
14 = 30 —	9 = 00	14 = 30 —	9 = 30
14 = 30 —	10 = 00	14 = 30	11 = 00
14 = 30	11 = 30	14 = 30	12 = 00
14 = 30 +	12 = 30	14 = 30 +	1 = 10
14 = 30 +	1 = 30	14 = 30 +	1 = 40
14 = 30	3 = 30	14 = 30	2 = 45
14 = 30	4 = 10	14 = 30	5 = 00
14 = 30 —	5 = 30	14 = 30 —	6 = 00
14 = 20	6 = 18	14 = 25	6 = 20
14 +	7 = 8	14 = 20	6 = 30
14 = 00	7 = 50	14 = 15	6 = 35
14 = 00	8 = 15	14 = 10	6 = 40
14 = 15 +	8 = 20	14 = 10 —	6 = 45
14 = 15 +	8 = 40	14 = 5	6 = 49
14 = 15 +	12 = 15	14 = 00	6 = 57

°	′	h.	′
14	= 00	12	= 27
14	= 00	12	= 32
14	= 00	12	= 35
14	= 00	12	= 43

Wind at S. W.

°	′	h.	′
14	= 15 +	8	= 20
14	= 20 −	8	= 30
14	= 25 +	9	= 00
14	= 25	12	= 12
14	= 25	12	= 21

Day warm, cloudy in the morning, evening clear.

April 19.

°	′	h.	′
14	= 30 −	8	= 35
14	= 30 −	9	= 00
14	= 30 −	1	= 30
14	= 30 −	2	= 00
14	= 30	3	= 30
14	= 30	4	= 00
14	= 20	5	= 00
14	= 25	5	= 38
14	= 25 +	5	= 45
14	= 30 −	6	= 00
14	= 30 −	6	= 45
14	= 25	7	= 00
14	= 20 −	8	= 00
14	= 20	9	= 00
14	= 20 +	10	= 00
14	= 25	11	= 00
14	= 25	11	= 15

°	′	h.	′
14	= 00	7	= 10
14	+	7	= 20
14	= 5 +	7	= 30
14	= 10	7	= 45
14	= 15	8	= 00
14	= 35 +	1	= 52
14	= 40 +	2	= 30
14	= 20	3	= 30
14	= 25	3	= 45
14	= 30 −	4	= 00
14	= 25 +	6	= 45
14	= 20	7	= 00
14	= 30 −	7	= 35
14	= 20 +	12	= 50

Day cold, wind at east.

May 3.

°	′	h.	′
14	= 10	9	= 30
14	= 15	11	= 10
14	= 15 +	12	= 40
14	= 15 +	2	= 20
14	= 10 −	5	= 20
14	= 10	6	= 5
14	= 10	6	= 45
14	= 10 −	7	= 5
14	= 5	7	= 15
14	+	7	= 30
14	= 00	7	= 42
14	+	8	= 00
14	+	9	= 38
14	+	10	= 15
14	+	11	= 00

Day warm, wind at east, some thunder in the afternoon.

May 2.

14° = 25′		9h = 30′
14 = 30	+	10 = 30
14 = 35	−	11 = 30
14 = 35	+	1 = 00
14 = 10		4 = 55
14 = 10		6 = 00
14 = 00		8 = 15
14 +		1 = 00

Wind at east.

May 5.

14° = 10′	+	9h = 30′
14 = 15		10 = 45

Day cold, wind easterly.

May 4.

14° = 5′		9h = 15′
14 = 5		9 = 30
14 = 10	+	1 = 35
14 = 10	+	3 = 17
14 = 10	+	3 = 50
14 = 15	+	12 = 30
14 = 20		1 = 57
14 = 20		2 = 45
14 = 20		3 = 25
14 = 20	−	4 = 35
14 = 15	+	5 = 30
14 = 15	+	6 = 10
14 = 15	−	12 = 7

Day clear, wind at east.

All these observations were made with the lightest of the three needles, the compass-box remaining unmov'd the whole time. From *February* 6. 1722. to the 10th of *May* following, Mr. *Graham* made upwards of 1000 observations in the same place; and the greatest variation westward was 14°=45′ and the least — — 13°=50′. It was seldom less than 14°, or greater than 14°=35′

Observations on Dr. Eaton's Stiptic; *by Dr.* Sprengell, Phil. Trans. N° 383. p. 108.

THE method of curing fresh wounds in a few days, without suppuration, where neither nerves, large vessels, bones, or any of the *viscera* are concerned, has been long ago observ'd: *Purman*, a famous surgeon of *Breslaw*, in his *Chirurgia curiosa* tells us of a mountebank, who gave himself 13 wounds, by incision, in the upper part of his left arm; and thereupon applied his *nostrum*, and with the help of a good roller, he was cur'd in two days time.

Next he mentions a martial ſtiptic, which ſtopp'd bleeding incomparably well, and heal'd freſh wounds (as he affirms) in two days; eſpecially if the patient took withal a few drops inwardly. This is likewiſe mentioned by *Blegny* near 30 years before.

When Dr. *Sprengell* was in *France*, he found that ſeveral trials were there alſo made, with a ſtiptic ball, mix'd with *French* brandy, by ſtriking a cock thro' the head, opening the crural artery of a dog, or chopping off a dog's leg, *&c.* But he found it did not amount to any thing of conſequence; yet he had ſtill a ball, made upwards of 20 years before, of filings of iron and an equal quantity of tartar, mix'd with *French* brandy upon a marble: This, with ſome alteration, was afterwards publiſh'd by the famous *Helvetius*, phyſician to *Lewis* XIV. of *France*, in a book, entitl'd, *Recueil des methodes pour la gueriſon des diverſes maladies*, which was reprinted in *Holland* in the year 1710. This preparation was then ſold by *Pierre Rottermond*, apothecary at the *Hague* in *Holland*.

The recipe for his medicinal ball is as follows. Take four pounds of the filings of ſteel, and eight pounds of tartar, well powdered; mix theſe well together. and put them in a new earthen pot, and pour thereon as much *French* brandy, as will make it into a poulteſs; let this ſtand fermenting in a cellar for four days, and ſtir it between whiles; then put it into *balneo mariæ*, and diſtill it *ſ. a.* with a moderate fire, to draw off the brandy: When you find, that nothing but the phlegm comes over, remove it from the fire and take out the maſs, ſtamp it very fine, that there may not remain the leaſt lump; then mix it again as before with a ſufficient quantity of brandy, and put it into the cellar to ferment, as before, and then diſtill it a ſecond time. This operation may be repeated ſeven or eight times: But the laſt time mix your maſs well upon a marble, and make it into two ounce balls. One of theſe balls is ſteep'd in a pint of good *French* brandy, a little warm'd, and hung only in it by a wire, till the brandy have receiv'd the colour of the ball: But if you are in haſte, then grate a ſufficient quantity of the ball in ſome brandy, ſtir it well, and you may uſe it that very inſtant.

The author, undoubtedly, thought by often grinding, fermenting and diſtilling this maſs, to comminute and ſubtilize its particles, ſo as to make it more fit to contract the fibres and veſſels of a wound, and to prevent ſtagnations of the fluids, both within and without, upon contuſions; but the ſucceſs

ſucceſs did not anſwer, and therefore it was laid aſide. Neither did *Helvetius* ever recommend it as an univerſal ſtiptic, aſtringent or conſolditating medicine; but merely in freſh wounds, and that only for a firſt dreſſing, and where people liv'd at a diſtance, and could not get immediate aſſiſtance from a ſurgeon. Beſides, he makes ſeveral exceptions, where it ſhould not be us'd; and in general, adviſes where chalybeate medicines may be made uſe of according to experience.

But as a balſamic ſtiptic was publiſh'd by one Dr. *Eaton*, for ſtopping all manner of bleeding, either external or internal, and that without any manner of exceptions; this made Dr. *Sprengell* deſirous to ſee it, and ſoon after he had an opportunity of examining it: He immediately found, that this was the ſame old medicine, which was alſo got into *England*, after other countries had diſcarded it. But he neglected it at that time, as not worth his notice, till he ſaw a treatiſe of conſumptions, publiſh'd by Sir *Richard Blackmore*, giving it the greateſt encomium, that ever was given to any invention whatever. 'For, *ſays he*, Dr. *Eaton*'s balſamic 'ſtiptic bids fair for the credit of a certain remedy in ſtop'ping of blood outwardly or inwardly, where the *craſis* of the 'blood is not entirely ruin'd, and will be of more ſervice to 'mankind, than all the diſcoveries made by *Galenical* com'pounders of drugs and ſyſtematical methodiſts.'

Dr. *Sprengell*, finding this remedy recommended in ſo extraordinary a manner, by ſo eminent a phyſician, began now to think, tkat poſſibly he might have been miſtaken, and therefore deſir'd Mr. *Winterbottom*, an apothecary in *Bow-lane*, immediately to prepare the recipe, as preſcrib'd by Dr. *Helvetius*. When this was ready, he ſent for a little of Dr. *Eaton*'s ſtiptic, and tried them both with galls before ſeveral gentlemen; when the tincture was the ſame, *viz.* a deep purple. He then precipitated the contents with old hock, and found the precipitated matter to be the ſame in both. Dr. *Sprengell*, not contented with his own enquiry, ſent ſeveral ſmall quantities to others, and went himſelf to Mr. *Godfrey*, the chemiſt: And they all told him, there was no difference. The Dr. tried ſeveral ways to find out its balſamic quality (whence it has its name) but found none. Upon which he was ſurpriz'd, that a man who had a mind to vend a thing, as a ſecret, had not done ſo much as to alter it, either in taſte, ſmell, or colour; and yet this might very well

well have been done, without robbing it of its virtue in the least.

The Dr. next tried these two upon the crural artery. Having procur'd a good middle siz'd dog, Mr. *Ranby* laid the artery bare, and open'd it with a lancet lengthways, for near half an inch. The old method was cutting the artery a-cross; and then there was no necessity of any stiptic at all, nor indeed here neither. But at first *Helvetius*'s tincture was applied, which stopp'd the bleeding; then opening the artery again, Dr. *Eaton*'s was tried with the same success: Afterwards the Dr. had the artery open'd in the other thigh, and trying it only with *French* brandy, he found this did as the other two. He opened the artery again, and dissolving a little *sal martis* and *saccharum saturni* in *French* brandy, he applied that, which answer'd in the same manner. This made the Dr. immediately conjecture, that there was but little virtue in either of them; but only that the brandy, by its great heat, did merely contract the fibres of the artery, which, no doubt, might be somewhat assisted by the steel; but this could not be much. The Dr. then reflected on the smallness of the crural artery in a dog, and not to be compar'd with that of a man, and that a little pledget of lint might have stopp'd the blood, without more to do, as well as the temporal artery, when opened with a lancet, which was done, and the pledget of lint stopp'd it accordingly. Thus far as to its external use. If only, according to *Helvetius*, it had been ordered to be taken internally, in fresh wounds and contusions, this might have been sufferable: But when, without exception, Dr. *Eaton*, as also Sir *Richard Blackmore*, recommended it in all outward bleedings, Dr. *Sprengell* thought proper to make some animadversions thereon. For, Sir *Richard* himself says, in his *Treatise of Consumptions*, p. 99. 101. that in spitting of blood, there is an orgasm, or stimulating ferment; What is this but a feverish indisposition? And is there any hemorrhage without it? Now if so, will not brandy and chalybeats heighten it? Which, by their heat and *stimulus*, brace and irritate the fibres, and accelerate the motion of the blood. And will not the blood then take up more room, and press harder against the sides of the vessels, and whatever opposes it? Is not this the way to cause an orgasm and hemorrhage?

Dr. *Eaton* himself tells us in his book, p. 57. That it did very much over-heat a gentlewoman, and that her bleed-

ing

ing ſtill continu'd after the taking it, and ſhe might hav periſh'd, had not a ſurgeon given her a cooling and aſtringen apozeme. And but juſt before, p. 47. he complains of phyſician that was not willing his patient ſhould take it, wh had a hectic fever upon her; becauſe he was afraid it was to hot.

Since the former trials, the Dr. deſired Mr. *Ranby* to ope the carotid artery of a dog, thinking that this artery migh give him more ſatisfaction than the crural arteries had done to try the ſtiptic qualities of *Helvetius* and *Eaton*'s tincture Having laid bare the jugular vein, divided it and tied it, tha its bleeding might not hinder the finding the carotid artery Upon opening the artery with a lancet, the blood ſpoute forth, and applying *Helvetius*'s tincture, the bleeding ſtopp'd He took it off in leſs than a minute, and made it bleed again but it bled but little, and then he applied Dr. *Eaton*'s ſtiptic He fill'd up the wound with lint, ſtitch'd up the *cutis*, the untied the dog and let him looſe; when, ſome time after, th Dr. ſeeing him, found he had bled a good deal, and was ſtil bleeding. The Dr. was very well ſatisfied, that the arter being ſo very ſmall, he would not bleed to death, and if h had nothing but lint upon it, it would have done as well It is to be obſerv'd, that the carotid arteries are largeſt in pro portion in human bodies, and that this artery of the dog wa but a ſmall matter bigger than the crural arteries of the for mer dog. This ſhews, that the ſtiptic quality of theſe tinctures is very inconſiderable; and that *Helvetius*'s tincture is rather better than Dr. *Eaton*'s, if there is any difference a all; tho' that, he believes, was owing to the brandy; for Dr. *Sprengell*'s brandy was ſtronger than Dr. *Eaton*'s.

The Dr. enquired for the dog next morning, and found him alive and well, only hanging his head on one ſide, which was owing to the muſcles being cut thro'.

The Specific Gravities of ſome Bodies; *by M.* Fahrenheit Phil. Tranſ. N° 383. p. 114. *Tranſlated from the* Latin.

Gold	19081
Mercury	13575*
Lead	11350
Silver	10481
Swediſh copper	8834
Japoneſe copper	8799
Iron	7817

Malacca

Malacca tin	7364
English tin	7313
White marcasite	9850
Regulus of antimony	6622
Brass	8412
Rock-chrystal	2669
Homogeneous pyrites	2584
Potash depurated from the filth, and a neutral salt, in which it more or less abounds	3112
The above neutral salt	2642
Sea salt	2125
Nitre	2150
Alum	1738
Very white sugar	$1608\frac{1}{2}$
Oil of vitriol	$1877\frac{1}{2}$*
Lixivium of potash, impregnated with as much salt as was possible	1563 *
The same prepar'd at another time	$1571\frac{1}{3}$*
The better sort of *aqua fortis*	1409*
Spirit of nitre	$1293\frac{1}{2}$*
Rain water	1000*
Rape-seed oil	913
Alcohol of wine	826
The same more dephlegmated	825

M. *Fahrenheit* made the experiments several ways; for, he weigh'd fix'd bodies, as is commonly done, first in a nice pair of scales in air, and then in rain water: He discover'd the weight of salts first in air, and then in a proper liquor, after which he compar'd it with the gravity of water; he discovered the gravities of the liquors, sometimes by the areometer, describ'd in a following *Transaction*, and sometimes in vessels, as here delineated.

A concave glass ball (as represented A Fig. 7. Plate XI.) blown pretty large at the flame of a lamp, furnish'd with two small glass tubes opposite to each other, as BB; the extremities of which were open, tapering, and somewhat incurvated, that the liquor might not run out. Besides, the ball was made somewhat flat in its lower part, that it might fit the better on the scale.

The bolt-head A Fig. 8. blown of very thin glass at the flame of a lamp, had a pretty large neck, whose aperture was shut with the lid B, made as accurately concave on the inside as possible.

By means of this bolt-head, the ſpecific gravities of ſalts may be found in the following manner. In the firſt place the bolt-head is fill'd with a proper liquor (in which the ſalt, whoſe gravity is ſought, will not diſſolve) and when the weight of the liquor is known, it is pour'd out, and the veſſel well dried: After this it is almoſt fill'd with the ſalt, whoſe weight is ſought; upon knowing which, the interſtices of the ſalt are fill'd with the liquor, and the increaſe of weight by the additional liquor, is ſought: if this increaſe of weight be ſubſtracted from the whole weight of the liquor, the remainder will give the gravity of the liquor excluded by the ſalt.

The neutral ſalt of potaſh cauſes no ebullition in ſpirit of nitre, it precipitates mercury, diſſolv'd in ſpirit of nitre, of a white colour; and when laid upon live coals, it decripitates and flies into ſmall pieces.

The nitre was melted in a crucible over the fire, that it might be freed of all its moiſture; and that the interſtices, filled with air, might be fill'd with the nitre itſelf.

The gravities of the liquors, mark'd with an aſteriſm, are reduced by calculation to the 48th degree of M. *Fahrenheit's* thermometers; and ſome of them were mentioned in the experiments on the degrees of heat in boiling, in *Phil. Tranſ.* N° 381.

The moſt ſimple method of finding the difference of gravity, which ariſes from the different temperament of fluids, is, firſt to fill ſome veſſels with a liquor not very hot (but whoſe degree of heat ſhould be diſcoverable by the thermometer) and then to weigh it; and afterwards to fill the ſaid veſſel again with hotter liquor, and weigh it as before. If the degree of heat in this ſecond experiment be again mark'd, you have the difference of the gravity of the liquor, cauſ'd by an intermediate degree of heat, which may then be eaſily calculated for every other degree.

The experiments were made in air; its gravity therefore is to be added to each number, in order to have the gravity of the liquors in *vacuo.* But the ſpecific gravity of air is to that of water, as 1 to 1000 nearly.

Some

Some new Experiments concerning the different, and sometimes contrary Motion of the Sap *in* Plants; *by Mr.* Fairchild. Phil. Tranſ. N° 384. p. 127.

MR. *Fairchild* ſhew'd ſome experiments before the Royal Society, which were allow'd to be new and uſeful; this made him try farther experiments, in order to ſhew the courſe of the ſap, which he found by experience ſo uſeful, that he could make barren trees fruitful, decaying trees healthful, and render the ſyſtem of gardening and planting more ſerviceable to the public.

He ſhew'd the *laureola*, grafted upon the *mezereon*, and the *ever-green* oak of *Virginia* upon the common *Engliſh* oak; both which hold their leaves all the winter, and are in a good ſtate, and flouriſhing, tho' grafted on plants that drop their leaves in winter; which plainly proves, that the juices riſe upwards, in winter, in thoſe plants that drop their leaves, otherwiſe the *ever-greens* that are grafted on them, would ſoon periſh.

Mr. *Fairchild* takes it, that by grafting the various foreign oaks upon the *Engliſh*, the timber might be made the more firm and laſting, than it is in its own nature, when rais'd from foreign acorns: For, as the crab-ſtock makes the wood of the apple-tree more firm and laſting than that on the apple-ſtock; and the peaches and almonds, budded on plumbs, are more laſting than thoſe on peach-ſtocks; ſo by the contrary rule, all firm timber, grafted on ſpongy ſtocks, would be made worſe than it would on its own bottom. For inſtance, if that which is call'd the *Engliſh* elm ſhould be grafted on that which is call'd the *Dutch*, it would partake a good deal of the ſpongy juices of the ſtock, whereby the timber would become unfit for the purpoſes it is now us'd for.

The firſt experiment Mr. *Fairchild* had to offer, was made on the *New England* cedar, or rather juniper, grafted on the *Virginia*; and what was remarkable in it was, that the branch which was grafted was left ſeveral inches below the grafting, which part continued growing, as well as the upper part above the grafting.

The ſecond was on the *viburnum*, whoſe top being planted in the ground, became roots; and the roots, being turn'd up, became branches. He found the plant in as good a ſtate of growing, as it was in its natural ſtate.

The third was on a pear-tree, which he enarch'd upon two pear-stocks in *March* 1721-22, and was then in a good flourishing state, with a branch in blossom, and receiv'd no nourishment but by the two enarch'd branches, the root being out of the ground; and tho' it was done upwards of two years before, it was then shooting suckers out of the root, which proves that the branches are as useful to support the roots, as the roots the branches; and it is, therefore, no wonder, that so many trees miscarry in planting, when there are no branches left on the head.

The fourth was on the cedar of *Libanus*, grafted on the *laryx*, which drops its leaves in the winter; yet maintains the cedar in as flourishing a condition, as if it had been on a tree, that held the leaves all the winter; and the part of the graft left below the grafting was in as good health, as the part above it.

An extraordinary Cure by Sweating *in hot Turf; together with a Description of the* Indian Hot-houses; *by Mr.* Paul Dudley. Phil. Transf. N° 384. p. 129.

IN 1704 *Peter Coffin* Esq; of *Exeter* in *New England*, being then 74 years of age, had taken a great surfeit as it was supposed, by drinking cold water in a very hot day, after having heated himself in the woods. This surfeit settled chiefly in his right side, but gave him a racking pain all over his body, and particularly deprived him of the use of his right arm. In this condition he kept his bed for nine weeks, and his recovery, considering his age, was despaired of; when his son (from whom Mr. *Dudley* had the account) proposed sweating him in turf. The father readily agreed to it, having used several medicines, without any effect. Immediately orders were given to cut a large oven full of turf, the pieces being about 18 inches square. The turf itself was of *English* grass, and only the swerd or top of the earth with the grass. Before the turf was put into the oven, the Dr. rubbed the grassy side of the turf with some spirit or oil (but of what sort the Dr. would give Mr. *Dudley* no account) and then doubled the grass-sides together, and so set them in. When they were well baked, which was in about two hours, he took them out and made a bed of them upon the floor (the place for the head raised) as soon as that was done, he ordered his father to be taken out of his bed without his shirt but wrapped up in a sheet, and laid upon the hot turf, and

and then he proceeded to cover him over with the rest of the turf, more especially on his side, where the seat of his pain was, but they laid none upon his breast or head; then they covered him with blankets to keep the heat in. While the father was in this bath, the son gave him warm cordials, to prevent fainting, which he was in great danger of: After he had lain thus about $\frac{3}{4}$ of an hour, which was as long as he could bear it, he was put naked into the bed, very well warmed, where in a few minutes he fell asleep, and sweated to that degree, that it ran thro' his pillow and bed, upon the floor. After about two hours sleep they dried him and put him on warm cloaths, and he found himself much eased and refreshed: This was in the morning; and before night he walked about the house, his pain being in a manner all gone. The next day the Dr. repeated his cordials; and the fourth day he sweated him a second time, in the same manner as above; and next day he went abroad about his business, and lived 11 years after, in perfect health and free from pain. The Dr. told Mr. *Dudley*, great care must be taken, that the patient do not lye too long in the turf; and even $\frac{1}{4}$ of an hour may be sufficient for some patients; and whenever the patient begins to fetch his breath short, or to faint, he must be put to bed immediately; and the physician or operator must by no means omit his cordials.

Sweating houses were common among the *Aborigines*, when the *English* first came into *New England*; tho' now but little used. A gentleman of the island of *Nantucket*, where the *Indians* sometimes practise it, even at this day, or very lately, gave Mr. *Dudley* the following relation. The cave was usually four foot high, and eight foot in diameter; the roof supported with sticks or boards, covered with earth, and they dug it in the side of a hill, and as near as possible to some river, pond or place of water. The entrance into this cave was small, and the door (when any person was sweating) was covered with a blanket or skin; near the cave they made a good large fire, and heated a parcel of stones, to the quantity of 500 weight, and rolled them in red hot, piling them up in the middle of the cave. When this is done, the *Indians* go in naked, and as many as please sit round the heated stones; as soon as they begin to grow faint, which may be in $\frac{1}{4}$ of an hour, they come out and plunge themselves all over in the water for a minute or two; and then in again, as long as they can well bear it; and so into the water a second time and then they dress themselves. This has been used with success for colds, surfeits, *sciatica's* and pains

pains fixed in the limbs; and even the *English* have ſeveral times found relief by it.

Mr. *Dudley* did not underſtand, but that it might be practiſed at any time of the year, without hazard, or inconveniency. The *Indians* often uſed it both before, and after long journies, hunting or voyages, to ſtrengthen and refreſh themſelves.

An Attempt to account for the riſing and falling of ſome Ponds near the Sea, or ebbing and flowing Rivers, where the Water is loweſt in the Pond, at the time of high Water, and higheſt in the Pond, at the time of low Water in the Sea or River; as alſo for the increaſe or decreaſe of the Water of ſuch Pools and Brooks, as are higheſt in dry and loweſt in rainy Seaſons, with Experiments to illuſtrate the ſolution of the Phænomena; *by Dr.* Deſaguliers. Phil. Tranſ. N° 384. p. 132.

HERO *Alexandrinus* and other hydraulic writers, have deſcribed a cup (called a *Tantalus*, from its effect) which will hold any liquor very well, when it is not filled above a certain height, marked in the cup, but if it be filled higher, not only the liquor above the mark will run out, but the whole liquor contained in the cup. This is performed by a ſyphon in the cup, which is ſometimes concealed to make the effect the more ſurpriſing.

The cup A B (repreſented Fig. 9. Plate XI.) has a viſible ſyphon C E D therein; the cup (Fig. 10.) has the ſame, conceal'd by the figure of a man, repreſenting *Tantalus* in the fable; and the cup (Fig. 11.) has its ſyphon more concealed, as it is carried up into the handle. Any of thoſe cups will hold water very well, provided they are not filled up above the line F G; for, then not only the liquor above F G will run out; but all the liquor in the cup, as low as D, the orifice of the ſhort leg of the ſyphon.

Experim. 1. In the veſſel *a b c d* (repreſented Fig. 12.) is placed an open wooden box A B C D, filled with water as high as the line L M; another box or plug E F G H made tight and containing weights to ſink it, is made to let down into the water between the partition I K and the end A B of the abovementioned box; but when it is not to preſs the water up to I O (as it does when let down) it is drawn out of the water by the weight *m*, which pulls it up by the bar *i k*, faſtened to a lever, moving round the centre *l*.

When

When by means of the plug, the water in the ſpace A B K I is puſhed up to I O, by paſſing under K; it runs out thro' the ſpout P Q (whoſe paſſage is gaged by a little ſluce P *p*) and falls into the veſſel R S, made of an oblong figure like a fiſh-pond, and having a ſyphon at S, ſo as to make it a *Tantalus*, or in the nature of the cups abovementioned.

Let the weight *m* pull up the plug E F G H, and the water, having filled R S, will run down below the orifice P to M.

The *Tantalus* R S beginning to run out, as ſoon as full, will for the abovementioned reaſons, continue to run till it be all emptied; and as it diſcharges itſelf into another *Tantalus* T V (whoſe ſyphon is at V) this laſt *Tantalus* will alſo, when full, begin to run out, and its water go down to X Y *a*.

If the plug be let down gradually, as ſoon as the water begins to run out of the laſt *Tantalus* T V (and the firſt *Tantalus* R S be covered, ſo as to be concealed from ſight) it will appear to the ſpectators, that the cavity T V, repreſenting a pond near an ebbing and flowing river (as the Dr. was credibly informed there is ſuch an one at *Greenhith* in *Kent*, between *London* and *Graveſend*) always riſes, whilſt the water at N O (or the tide) falls to L M; and always ſinks, while the water at L M, or the tide, riſes to N O.

Exp. 2. Let the water in the box A B C D not be made uſe of; only the veſſel Z filled every half hour; it will empty itſelf in a ¼ of an hour, falling like rain and dropping thro' the leaden platform *ef* into the concealed *Tantalus* R S, which will not begin to run, till this artificial rain be over. Then in ¼ of an hour more, the *Tantalus* R S will have emptied itſelf into the viſible *Tantalus* T V, which will be filling all the time after Z has done running; or in the dry ſeaſon and as ſoon as T V is full, it will begin to run out thro' its ſyphon V, at the end of the half hour, when the veſſel Z or ſieve runs again; that is, at the return of the rainy ſeaſon.

This laſt experiment may be eaſily applied to thoſe ponds or brooks, that are high in dry weather, and low in wet weather; of which kind the Dr. was told there is a brook at *Lambourn* in *Berkſhire*.

If it be objected, that ſuch ponds are full for ſome time, which a *Tantalus* cannot be, becauſe it begins to run out as ſoon as full; that may be eaſily ſolved by ſuppoſing the hidden *Tantalus* (or intermediate cavity between the river and pond) to contain more water than the viſible one, provided it does not

contain

contain so much, as not to be emptied, before the return of the tide.

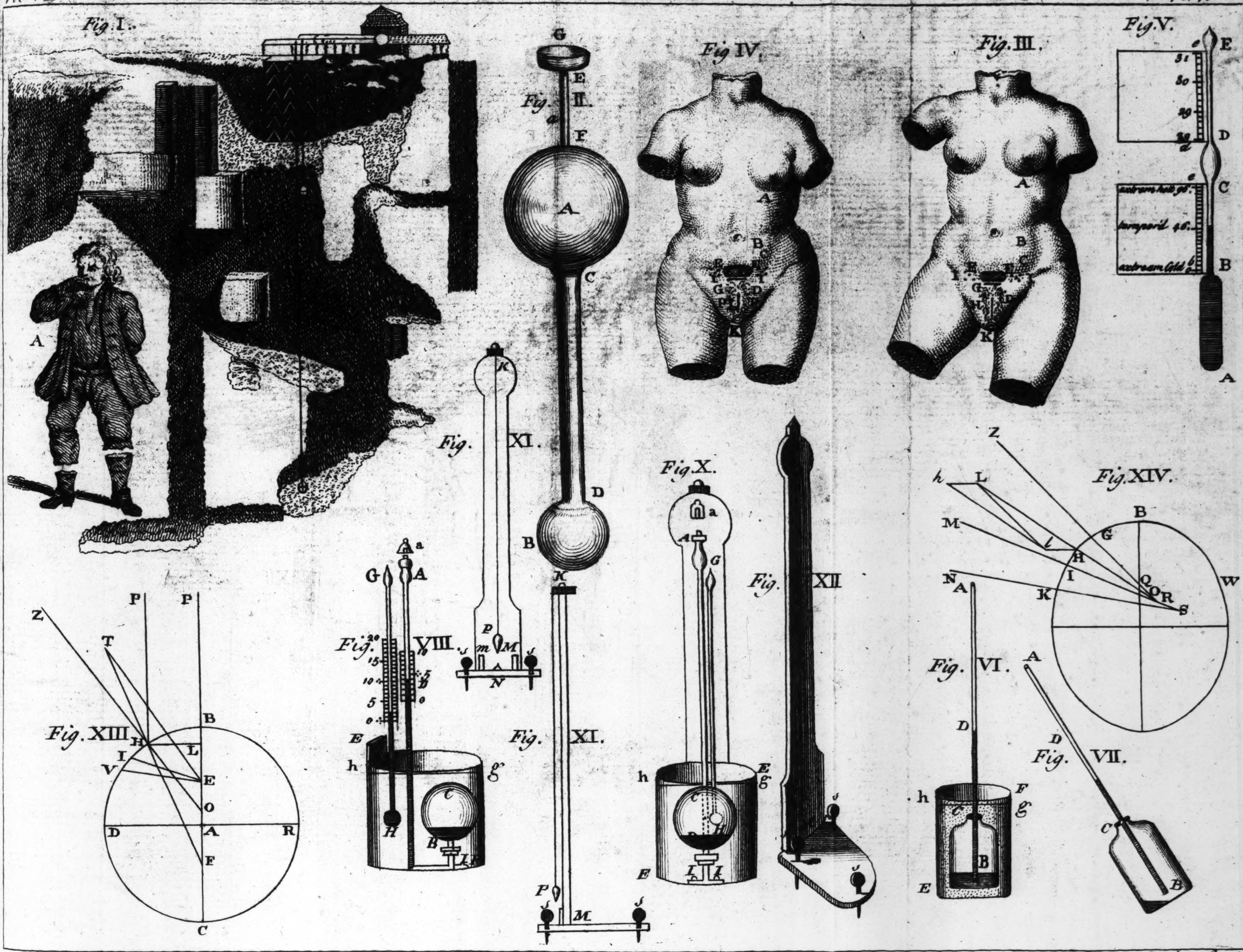
The same solution will serve for wet and dry seasons; only supposing the cavities larger.

If it be asked, where the water of the visible *Tantalus*, near a river can run; it may be answered, that all this may happen, tho' the second, or lowest *Tantalus*, should have its bottom higher than low water mark in the river; and as to the syphons, which are of a particular make in the cup; tho' such be not supposed to be in the earth; yet any long passage, rising in the middle, will answer the end. A B C D (Fig. 13.) represents the channel of a river; A D high water mark; and G H low water-mark; Z I a passage from the river to the cavity I K L M N, or first or hidden *Tantalus*; L M Q the syphon of the first *Tantalus*, running into the second *Tantalus*, or visible pond O Q R P, which by its syphon R S V runs out into low grounds, that may be above the low water-mark G H; and the bottom K L of the first *Tantalus* may be above the top of the last, whose level is the line W W; A B C D Y O Q R P V H is the section of the surface of the earth.

The Body of a Man *found in a* Copper Mine; *by M.* Adam Leyel. Phil. Transf. N° 384. p. 136. *Translated from the* Latin.

IN *December* 1719, in the *Wredian* shaft, 82 fathoms deep, was found the body of a man, 5 fathoms under water, and the ruins of a rock; which falling in upon him had crushed both his legs, his right arm and head; but his face and the rest of his body and his cloaths were whole and entire: A Fig. 1. Plate XII. represents him with his left hand stopping his mouth with the end of his neck-cloth; his purse and a long brass-box, in which was a piece of tobacco, were sound and entire; but the water impregnated with vitriol had entirely consumed the piece of iron wire on which the lid of the box turned. His flesh and skin tho' they felt rough and hard to the touch were not petrified, but resembling horn or hoof; for, it might be cut with a knife.

After the body was taken up, there was diligent search made to find if any knew the man, or the time he perished; when one *Magnus Johannis*, a miner in *Korsgården*, knew him very well by face (all the lineaments of which were entire) and added, that his name was *Matthias Israëlis*, or, on account of his



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his ſtature, *Great* or tall *Matthias*, born at *Boda* a village in the pariſh of *Swerdſiöen*, and employed by *Jonah Petri* in *Dijkarebacken*; and moreover he added, that going down by himſelf in autumn 1670 into the ſhaft, he was afterwards miſſing, and was undoubtedly ſmothered in the ruins; this account was confirmed by ſeveral others, and particularly by an old woman, to whom *Matthias* had been contracted.

He had therefore lain under ground for 49 years, *viz.* from 1670 to 1719; and the body is ſtill preſerved in a houſe belonging to the mines, where to this day it is to be ſeen entire and without any ſtench; not only his cloaths and linen, but his fleſh, ſkin, hair and nails, were preſerved from putrifaction by means of the water impregnated with vitriol.

It appears from this account, as given by the people living thereabouts, that the body was not petrified, but indurated by the vitriolic water: For, nothing is more contrary to the nature of vitriol than a petrifying quality; but by the briſk motion of a very fine vapour it pervades, conſtringes and preſerves every thing from putrifaction.

The Deſcription and Uſe of a New Areometer; *by M.* Fahrenheit. Phil. Tranſ. N° 384. p. 140. *Tranſlated from the* Latin.

THE two tubes CD and EF (Fig. 2. Plate XII.) are joined to the ball A which is pretty large (the larger the better) and a receptacle G to the ſmaller tube EF, and the middle of the tube is diſtinguiſhed by a very ſmall point *a*, yet ſufficiently diſcernible; the other extremity of the tube CD is furniſhed with a ball B, which ſerves inſtead of a receptacle to the inferior weight, with which the inſtrument is charged: Let the diſtance of the ball B from the centre of the ball A be thrice greater than the diſtance of the receptacle G from the ſame centre. The inſtrument being thus prepared, let the ball B be filled with ſo much mercury, that if the areometer be immerged into the lighteſt liquor, as for inſtance, ſpirits of wine, well dephlegmated, or ſpirits of turpentine, it may deſcend therein almoſt to the point *a*; after which, the tube is hermetically ſealed near E, and the inſtrument weighed in a nice pair of ſcales; and the weight of the inſtrument will be the very ſame with that of the liquor, excluded by the inſtrument, as is well known from hydroſtatics. But if the weight of heavier liquors, as water, *lixivia* or acid ſpirits, be ſought, their difference of gravity is found, when the inſtrument is charged with

ſo much weight in the receptacle G, as may make it deſcend again to the point *a*: Upon adding this weight to that of the inſtrument, the ſpecific gravities of theſe liquors (if the weights be very ſmall) will be had ſufficiently exact; and ſo of the reſt.

M. *Fahrenheit* affirmed that the inſtrument ſhould deſcend in the abovementioned ſpirits almoſt to the point *a*; but it will be better, that the liquor do not exactly reach that point, and that the inconſiderable difference be made up by very ſmall weights; for, in this manner, were there ſtill lighter liquors propoſed or were the gravity of the abovementioned liquors rendered ſpecifically lighter by heat, they might ſtill be diſcovered by the inſtrument, which otherwiſe would not happen, ſhould it deſcend in the ſaid liquors exactly to the point *a*.

In making the experiments, care is to be taken, that neither the ſuperficies of the inſtrument or of the liquors have any fat, or other heterogeneous particles adhering to them; elſe the experiments can never be accurately made.

A preternatural Structure of the Parts of Generation in a Woman; *by Mr.* John Bonnet. Phil. Tranſ. N° 384. p. 142.

AS there appears to be ſome difference in the accounts Dr. *Huxham* and Dr. *Oliver* gave of the parts of generation in this woman *Phil. Tranſ.* N° 379. Mr. *Bonnet* ſurgeon at *Foye* in *Cornwall* gives a particular account of what he obſerved, with reſpect to this matter, having ſeveral times obſerved the wonderful ſtructure of the parts both before the birth approached, and before he was obliged to make the inciſion.

This woman was about 23 years of age, when ſhe was married, and ſome time after, ſhe conceived. As ſhe was conſcious of the preternatural ſtructure of the parts and her mother apprehenſive of the danger that would attend the delivery under ſuch unhappy circumſtances, they applied to Mr. *Bonnet* about the ſeventh month in order to engage his aſſiſtance. Upon viewing the *abdomen*, he made the following obſervations: There was no ſign of the *umbilicus*; but about three inches lower than it is regularly placed in perſons naturally formed, there was a ſpongy fleſhy protuberance, nearly of the ſhape and bigneſs of a hen egg; not, as in Dr. *Oliver*'s account, compoſed of ſeveral lobules, envelopped in diſtinct membranes, but entirely reſembling that luxuriant fleſh, in ill-digeſted wounds, and therefore, commonly called proud fleſh. This was exceed-

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ing tender, and could not bear the leaſt touch: On the lower part of this excreſcence, he perceived two ſmall orifices about an inch diſtant from each other; thro' theſe the urine continually ouzed, nor could ſhe contain it, but by violent efforts could make it ſpout out near a foot. What Dr. *Oliver* ſays of its being rendred in ſeveral ſmall ſtreams is certainly falſe: The two orifices by which it really was diſcharged, being now very evident, and eaſily admitting a ſmall probe. About ¼ of an inch below this protuberance, was a tranſverſe orifice, very much reſembling the *anus* of a cock; thro' this the *menſes* regularly flowed; by this ſhe was impregnated. It was with ſome difficulty Mr. *Bonnet* thruſt his finger into this orifice, in order to reach the *os tineæ*, which, however, he could by no means feel, it lying ſo deep; but he plainly felt a thick tranſverſe membrane, ſeparating this paſſage from an orifice, ſituated about two inches below that already deſcribed. This lower orifice ſeemed to be ſituated exactly at the *ſymphyſis* of the *oſſa pubis*, in women regularly formed, ſomewhat above the place where the natural *hiatus* ſhould have been; he could but juſt enter the tip of his finger into this; there were a few hairs ſcattered irregularly up and down about this orifice. The *anus* terminated as uſual with a *ſphincter*, about two inches below this lower orifice, much more forwards than uſual; ſo that the upper orifice, which may be properly called the orifice of the *vagina*, was about ¼ of an inch below the umbilical excreſcence: The lower oblong orifice (or another paſſage to the *uterus*) was about two inches below that of the *vagina*, contrary to what is aſſerted in Dr. *Oliver*'s account, who makes the ſuperior orifice to be near four inches below the excreſcence, and the lower orifice four inches from the ſuperior; and then affirms, that the *anus*, which the Dr. (rightly enough) makes to uſurp the place of the *foſſa magna* in women naturally conformed, was five or ſix inches below this: According to this proportion, the woman muſt have been really gigantic; whereas, ſhe was a ſhort little woman; nor do Dr. *Oliver*'s cuts any ways repreſent the caſe. She had no *os* or *oſſa pubis*; it is true, there was an *apophyſis* jutting out from the lower part of each *os ilium*; but they were far from being joined, as uſual, by *ſynchondroſis*.

July 18, 1722. Mr. *Bonnet* was ſent for late at night: He found the woman with true travail-pains upon her; the throws were exceſſive violent, and the continued agony had almoſt

quite exhauſted her ſpirits; but the orifice of the *vagina* was no ways ſenſibly dilated; tho' the *anus*, thro' the violence of the throws, opened exceeding wide: Vain were all endeavours, by thruſting up the child, and putting the mother in a proper poſture: Vain were her own throws and agonies; convulſions now had ſeiſed her, and nature ſeemed to have denied a longer life to the mother, or an entrance into it to the child. Mr. *Bonnet* was in the utmoſt perplexity what to do under theſe circumſtances; on the one hand he conſidered, that if there was not a paſſage made for the child, and that by inciſion, both mother and child muſt ſpeedily periſh; on the other hand he foreſaw the hazard of an inciſion: At laſt he reſolved to try a doubtful method of preſerving life, rather than none at all. He told her mother and the other perſons in the room, that death was inevitable, without widening the paſſage by inciſion, and ſo attempting a delivery; withal apprizing them of the uncertainty of the event of the operation. When they ſaw her, as they thought, juſt expiring, they delivered her into his hands, to do with her what he thought fit. He immediately thruſt his ſcalpel into the inferior oblong orifice of the *vagina*, and directing it towards the ſuperior, he brought both into one; then preſently with his ſciſſars, he ſnipped the tranſverſe membrane: This being done, he eaſily introduced his hand, felt the head of the child, and thruſting his finger into its mouth, he extracted a female infant, alive and well formed, to his own ſurpriſe and that of all the aſſiſtants.

Ever after, ſhe was troubled with a *prolapſus uteri*, upon the leaſt ſtanding or walking: Mr. *Bonnet* propoſed to remedy this by a ſuture, as is practiſed in the caſe of the *vulva* breaking into the *anus*; but ſhe would by no means admit of it. So that ſhe almoſt continually laboured under a *procidentia uteri*, and the body of the *uterus* and *vagina* were ſo corroded by the acrimony of the urine, that there were four or five ulcers form'd upon them. Beſides this inconvenience, ſome of the thinner parts of the *fœces* were diſcharged at the bottom of this large *rima*, and by introducing his finger at the bottom thereof, he could eaſily thruſt the top of it thro' the *anus*; which ſhews, that there could not be five or ſix inches between the oblong orifice and the *anus*, as is aſſerted by Dr. *Oliver*; the diſtance being ſcarce above two inches.

A (Fig. 3. Plate XII.) repreſents the *epigaſtrium*; B the umbilical region; C the *hypogaſtrium*; D the *regio pubis*;

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EE the ſpongy maſs; G the tranſverſe orifice; H the oblong orifice; II two cicatrices, occaſioned, probably, by the acrimony of the urine; K the *anus* placed a little forwards.

FF (Fig. 4.) repreſents the urinary paſſages, which were diſcernible enough after the delivery; P P the *fiſſura magna* four inches long; L the *procidentia uteri*; M the *os tineæ*.

A Partial ſight of Objects; *by Dr.* Abraham Vater. Phil. Tranſ. N° 384. p. 147. *Tranſlated from the* Latin.

A Middle aged woman, was in one night ſeiſed with a black cataract or *gutta ſerena*, by the retropulſion of a *coryza*, by bathing and a ſubſequent cold: For, getting up in the morning, ſhe found herſelf deprived of her ſight, without any external blemiſh in her eyes. After taking ſeveral laxative medicines and purifiers of the blood, and at the ſame time applying veſicatories, and uſing a regular diet, her ſight gradually returned; yet with the following particular phænomena: At firſt all the people ſhe met, appeared without heads, their bodies only without the head preſenting to view. In proceſs of time ſhe ſaw objects entire, but thro' a miſt or net, as it were; and after this, ſhe obſerved ſpots or mots before her eyes: At length this diſorder degenerated into a partial ſight: And now ſhe ſaw an object entire, with both her eyes open and directed towards it; but ſhutting either of them, part of the object ſeemed to be covered with a round ſpot or miſt, which appeared ſo much the larger, as the eye was farther removed from the object: But as either the right or left eye was ſhut, the middle (tho' different) part of the objects was obſcured: Thus, for inſtance, if ſhe view'd with her left eye only theſe three words *ego ſum cœca*, the middle word *ſum*, when the pupil was directed towards it, was not ſeen, but only the words *ego — cœca*; and when the pupil was directed towards *ego*, it was not ſeen, only the words *— ſum cœca:* But if on the contrary ſhe ſhut her left eye and viewed an object with her right, then it is true, the middle of the object was not ſeen; yet in ſuch a manner, that only a fourth part of the object eſcaped the ſight, while the other three parts were diſtinctly ſeen. Thus, for inſtance, ſhould ſhe view with this eye theſe four words *ego opto eſſe ſana*, then the pupil being directed towards the middle, ſhe ſaw all the words but *opto*, which was obſcured by a round ſpot, and with a great ado ſhe ſaw *ego — eſſe ſana*.

Since, therefore, it appears from this caſe, that the round ſpot, by which part of the object was obſcured, was really fixed,

fixed, tho' changing its place according to the different direction of the pupil, it ſeems doubtful, whether the defect was in the cryſtalline humour or in the *retina?* In the *cornea* there appeared no ſpot or obſcurity.

The abovementioned phænomenon, *viz.* where ſhe could ſee the whole body of a man, excepting only his head, ſeems to be of greater conſequence and more difficult to be explained.

The Diſſection of two Eyes *with* Cataracts; *by Mr.* Molyneux. Phil. Tranſ. N° 384. p. 149. *Tranſlated from the* Latin.

ONE *John Wright*, an old ſoldier, having a cataract in both his eyes, at length loſt his ſight entirely. He applied to Mr. *Caywood*, occuliſt in *Dublin*; who couched both his eyes, in order to depreſs the cryſtalline humour: But the operation not ſucceeding ſo well on his right eye, he only recovered the uſe of his left. A few months after, he was taken into the King's hoſpital for decay'd ſoldiers near *Dublin*, where he continued for eight or nine years, and during all that time, his ſight was pretty good, ſo that he could walk by himſelf thro' the city, and diſtinguiſh the faces of his comrades. At length he died *April* 5, 1722.

Upon removing the *cornea* with a part of the *ſclerotica* of the left eye (the ſight of which he had recovered, as above) there was not the leaſt ſign of a pellicle, either floating in the aqueous humour, or adhering to the edge of the *iris*, as *Wolhouſius* affirms he had obſerved; and what is ſtill more ſurpriſing, there was nothing remaining of the cryſtalline humour, to whoſe opacity Dr. *Briſſeus* aſcribes this diſorder: Nothing preternatural was obſerved in the ſite and conſiſtence of the vitreous humour; the *choroides* and *retina* were of a duſkiſh colour both internally and externally: The ſame things were obſerved in the right eye, and no ſigns of a pellicle or cryſtalline humour: The *cornea* was rugous and flaccid; becauſe the aqueous humour, extravaſated by the puncture, had never been renewed, and therefore, the patient loſt the uſe of that eye entirely: The want of the cryſtalline humour, is a thing quite unheard of, no anatomiſt, as far as Mr. *Molyneux* knows, having ever obſerved or even ſuſpected any ſuch thing. He doubts not, but the patient had the cryſtalline humour in both eyes before the operation; but he ſuppoſes, by its being depreſſed by the needle, and thereby the *ligamenta ciliaria* and veſſels, by which it was nouriſhed, being entirely broken, it gradually

gradually wasted away. It were to be wished, that this matter were confirmed by repeated experiments: For, Mr. *Molyneux* suspects, that the same thing always happens in a certain time after depressing the crystalline humour.

From this account it is evident, that a person may see without the crystalline humour, contrary to the opinion of *Wolhousius*, and by it is farther confirmed the opinion of Dr. *Brisseus* concerning a cataract.

An Account of the Scarabæus galeatus pulsator, *or* Death-watch; *by Mr.* Stackhouse. Phil. Transf. N° 385. p. 159.

MAY 16, 1724. Mr. *Stackhouse* happened to hear what is called the *death-watch*; and as he cast a diligent eye over the sedge bottom of a chair, he hit luckily on the place where the insect was beating; so that it discovered itself to him by its own pulsations.

It lifted up itself upon its hinder legs, and somewhat extending or inclining its neck, it beat down its face upon the sedge, with great force and agility: The sedge, upon which he found it, was bared of its outer coat, for about the length of ½ an inch; the insect stood upon the inner bulbous part, and beat upon the outer coat, as if it had been working it off, as it went; the impressions of its strokes were very visible, the coat of the sedge being depressed, where it had beaten, for about the compass of a silver penny; whether it beat for exercise or food Mr. *Stackhouse* could not tell; but very probably it might be for the latter; and he is rather inclined to think so, because there were more than one such place upon the sedge, where it had been at work; and where, it is likely, it might have continued for some days.

As to what Mr. *Derham* has observ'd, *viz.* that the beatings are a sort of prelude for copulation, Mr. *Stackhouse* oould not discover, that this beetle had any other of the same species near it; and, therefore, he is inclin'd to think, that it beat for the preparation of its food, at this time at least, whatever it might do at other times for pleasure. The description Mr. *Allen* gives of the insect, as far as Mr. *Stackhouse* could find by this (which he took from the chair where he found it, and put it into a box) is very true: It is about ¼ of an inch in length, of a dark dirty colour, with a broad helmet over its head, under which, when quiet, it draws up its head; so that this helmet or *galea* is, when the insect rests, a very considerable defence against such falls as are frequent in rotten

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and decay'd places, which this inſect ſeems to frequent very much: The ſecond day after he took it, he opened the box wherein it was, and ſet it in the ſun: The inſect ſoon became very briſk, and crept backwards and forwards along the pieces of ſedge and rotten wood, he put with it into the box; till at length getting to the end of one of the pieces, it immediately ſtruck out its wings, and was juſt going to take flight; but having the lid of the box ready in his hand, he ſhaded it over and it ſoon drew in its wings and was very quiet. He could not before perceive, tho' he made uſe of a tolerable good glaſs, any the leaſt ſign of a fiſſure upon its back; and for that reaſon he queſtioned much, whether it had any wings or not, till he ſet it in the ſun. The head of this inſect appears to be of a very fine contexture, as it is ſeen, when it creeps about, and ſtretches it forward; but when it is drawn up under its helmet, it ſeems to be cover'd with a membrane thick ſet with fine hairs.

It liv'd about a fortnight; but he could never perceive that it beat, after it was confin'd in the box.

A new Contrivance for taking Levels; by Dr. Deſaguliers. Phil. Tranſ. N° 384. p. 165.

IT has been long ago obſerv'd, that the air thermometer is alſo a barometer; and becauſe the liquor in it will riſe and fall, as well by the change of the weight of the air, as by the rarefaction of the air by heat and cold, this inſtrument has been no longer made uſe of as a thermometer; and in its ſtead, ſpirit-of-wine thermometers, hermetically ſeal'd, have been us'd ever ſince.

But becauſe the errors of the air thermometers (or its difference from the ſpirit thermometer) depend only upon the change of the weight of the atmoſphere from what it was, when the two thermometers were ſet at the ſame degree of their reſpective ſcales; the late Dr. *Hook* contriv'd an inſtrument, which he call'd a marine barometer, made of a combination of the two above-mentioned thermometers; in ſuch manner, that a third ſcale being made uſe of, to obſerve the difference of the two thermometers, thereby the change of the gravity of the air; and conſequently, ſtormy, rainy, and fair weather might be foretold at ſea, where the quickſilver barometer becomes uſeleſs by the ſhaking of the ſhip.

Dr. *Halley* has in the *Philoſophical Tranſactions* propos'd Mr. *Patrick*'s pendent barometer for taking the level of diſtant

tant places; becauſe the mercury, in the tube of the ſaid barometer, does ſometimes riſe and fall a foot, or a foot and a half; if therefore, the motion of the mercury in this barometer be five times more ſenſible than in the common one, $\frac{1}{10}$ of an inch of fall of the mercury will anſwer to an height of 18 foot; and therefore, ſuch an inſtrument might be of uſe in taking the levels of diſtant places. But Dr. *Deſaguliers* found by ſeveral experiments, that this will not anſwer in practice; becauſe as the tube of ſuch a barometer is of a very ſmall bore, the attraction of coheſion, by which the mercury is apt to adhere to the tube, will diſturb the motion of the mercury, caus'd by the different preſſure of the atmoſphere; ſo that ſetting up this barometer ſeveral times ſucceſſively in the ſame place, it will often differ $\frac{1}{10}$ of an inch or more; and if it be ſhaken, as is commonly done to ſet it right, the mercury will ſometimes part, and a drop of it fall from the reſt; ſo that it is leſs to be depended upon for this uſe, than the common barometer.

Mr. *Stephen Gray* has often made a very ſenſible barometer. Into a little bottle CB (repreſented Fig. 5. Plate XII.) he fixes a tube AB of a very ſmall bore, open at both ends, and cemented tight to the neck of the bottle at C; then warming the bottle with the hand, in order to drive out ſome of the air, he immerges the end A into water, tinged with cochineal; ſo that as the air cools in the bottle CB, ſome of the red water is forced into the bottle; then ſetting the bottle upright again, the liquor in the bottle will ſtand at B, (above the end of the tube) and that in the tube at D; but if it ſhould ſtand higher or lower than D, it may be brought to that place by ſucking or blowing at A. If the inſtrument thus prepar'd, be firſt ſet on the ground, and a ſpringing ring of fine wire ſlipp'd on the tube down to D, by way of index, and then ſet upon any table, or other place, ſcarce a yard higher, one may obſerve that the liquor is riſen ſenſibly. The Dr. has obſerv'd it riſe $\frac{1}{4}$ of an inch, when the bottle was ſet but a yard higher than where it ſtood before; ſo that the column of atmoſphere, that preſs'd down the tube, whilſt the machine was on the ground, being ſhorten'd only three foot, was ſo overbalanced by the expanſion of the air in the bottle at B, that the liquor roſe $\frac{1}{10}$ of an inch above D. There is, it is true, a great uncertainty in this inſtrument: For, ſince it is a thermometer, as well as a barometer, the warmth of the hand that touches, or even comes near it, will make it riſe,

if the air in the bottle were cold before. Mr. *Gray*, therefore, contrived to put the bottle C B into the veſſel F E, which he fill'd with ſand, that in raiſing the inſtrument, and moving it up and down, the air in C B might continue in the ſame ſtate, and the machine be only a barometer during the experiment.

This ſeems to bid fair for an inſtrument, by which the different levels of places may be taken; but upon a nice examination it will be liable to error: For, tho' ſand be not alter'd in its heat or cold of a ſudden; yet in two or three hours, as it is carried into a warmer or colder place, it will become hotter or colder, and the leaſt degree of heat or cold, communicated to the air C B, will alter the height of the liquor at D, when the inſtrument is made ſo ſenſible, as the Dr. has mentioned. Then, if in carrying the inſtrument, it ſhould be accidentally inclin'd; ſo that the liquor in the bottle (repreſented Fig. 6.) ſhould not cover the bottom of the tube at B, ſome liquor may fall out of the tube, or ſome air may inſinuate into it; each of which accidents will quite ſpoil the experiment. But if this machine be made portable, without any inconveniency, and ſecur'd againſt the action of heat and cold (or which is the ſame thing, if the alterations by heat and cold be exactly allow'd for) it will be of very great uſe and certainty, in taking the levels of diſtant places, provided they be not ſo far diſtant from each other, as to require upwards of 6 hours time to carry the inſtrument from one place to another: Nay, very diſtant places, even at two or three days journey from each other, may be taken tolerably well with two inſtruments, nicely adjuſted to each other, if they be taken notice of by two obſervers at the ſame hour, in fair weather, and when there is no wind.

Now, Dr. *Deſaguliers* contriv'd ſuch an inſtrument, by which the difference of level of two places, which could not be taken in leſs than four or five days with the beſt teleſcope levels, may be taken in as few hours.

To the ball C (Fig. 7.) is join'd a recurve tube B A of a very fine bore, with a ſmall bubble a-top at A, whoſe upper part is open. It is evident from the make of this inſtrument, that if it be inclin'd in carrying, no prejudice will be done to the liquor, which will always be right, both in the ball and tube, when the inſtrument is ſet upright: If the air at C be ſo expanded by heat, as to drive the liquor to the top of the tube, the cavity A will receive the liquor, which will come

down

down again and ſettle at D or near it, according to the level of the place, where the inſtrument is, as ſoon as the air at C returns to the ſame temperament, as to heat and cold. To preſerve the ſame degree of heat, when the different obſervations are made, the machine is fixed in a tin veſſel F E, fill'd with water up to *g h*, above the ball; and a very ſenſible thermometer has alſo its ball under water, that one may obſerve the liquor at D in each experiment, when the thermometer ſtands at the ſame height as before. The water is pour'd out, when the inſtrument is carried, which one may do conveniently, by means of the wooden frame, which is ſet upright, by means of three ſcrews, ſuch as *s* (Fig. 8.) and a line and plummet *p* P. At the back part of the wooden frame, from the piece at top K (Fig. 10.) hangs the plummet P, over a braſs point at N; M *m* are brackets to make the upright board K *n* continue at right angles, with the horizontal one at N. Fig. 11. likewiſe repreſents the wooden frame and ſcrews. Fig. 9. repreſents a front view of the machine, ſuppoſing the forepart of the tin veſſel tranſparent; and here the braſs ſocket of the recurve tube, into which the ball is ſcrew'd, has two wings at I I, fix'd to the bottom, that the ball may not break the tube by its endeavour to emerge, when the water is pour'd in as high as *g h*.

After the Dr. had contriv'd this machine, he conſider'd, that as the tube is of a very ſmall bore, if the liquor ſhould riſe into the ball A, in carrying the inſtrument from one place to another, ſome of it would adhere to the ſides of the ball A; and that upon its deſcent in making the experiment ſo much might be left behind, that the liquor would not be high enough at D, to ſhew the difference of level; therefore, to prevent that inconveniency, he contriv'd a blank ſcrew to ſhut up the hole at A as ſoon as one experiment is made, that in carrying the machine, the air in A may balance that in C; ſo that the liquor ſhall not run up and down the tube, whatever degree of heat and cold may act upon the inſtrument, in going from one place to another.

Now, becauſe one experiment may be made in the morning, the water may be ſo cold, that when a ſecond experiment is made at noon, the water cannot be brought to the ſame degree of cold, it had in the morning; therefore, in making the firſt experiment, warm water muſt be mix'd with the cold; and when the water has ſtood ſome time, before it comes to be as cold, as it is likely to be at the warmeſt part

of that day; obſerve and ſet down the degree of the thermometer, at which the ſpirit ſtands; and likewiſe the degree of the water in the barometer at D; then ſcrew on the cap at A, pour out the water, and carry the inſtrument to the place whoſe level you would know; there pour in your water; and when the thermometer is come to the ſame degree as before, open the ſcrew at top, and obſerve the liquor in the barometer.

The Dr's ſcale for the barometer is 10 inches long, and divided into tenths; ſo that ſuch an inſtrument will ſerve for any heights, not exceeding 10 foot, each tenth of an inch anſwering to a foot of height.

N. B. The Dr. has made no allowance for the decreaſe of denſity in the air; becauſe he did not propoſe this machine for meaſuring mountains (tho' with a proper allowance for the decreaſing denſity of the air it will do very well) but for heights that want to be known in gardens, plantations and the conveyance of water; where an experiment, that anſwers to two or three foot in a diſtance of 20 miles. will render this a very uſeful inſtrument.

The bones of a Fœtus *voided by Stool; by M.* Bernard Shiever. Phil. Tranſ. N° 385. p. 172. *Tranſlated from the* Latin.

A Woman of 41 years of age, conceived in *July* 1720, and having gone ſeven months with child, tho' ſometimes ſhe had her *menſes* in a ſmall quantity, ſhe perceiv'd her belly leſſen, with only a kind of preſſure remaining in her right ſide. A month after, ſhe conceiv'd again, and in *December* 1721 was deliver'd of a dead female child, of a proper ſize. From that time ſhe kept her bed till *June* 1724. In *May*, happening to go to ſtool, ſhe felt a pain in the *anus*, as if the *rectum* would drop from her; endeavouring with her fingers to relieve herſelf, ſhe extracted a piece of the *crainum*, as big as a *Swediſh* crown, call'd a *dubble carolin*; and at the ſame time two ribs were found in the cloſe-ſtool; and 14 days after, the reſt of the bones, (of an excrementitious colour) were voided the ſame way, which M. *Shiever* himſelf ſaw and handled. The woman did afterwards very well, went about her family affairs, and was the mother of three children, ſhe alſo had her *menſes* naturally.

An Account of an Aurora Borealis, *observ'd at* Upsal, September 20. 1720; *by M.* Burrman. Phil. Transf. N° 385. p. 175. *Translated from the* Latin.

THE *chasmata cæli*, as they are call'd, or the *lumen horizontale* and *aurora borealis*, in *Swedish Nord-skjen*, *Nord-ljus*, *Nord-blyss*, *Nord-blâss*, *Lâterskjen*, *Lyssnor*, &c. by the vulgar suppos'd to be representations of armies and battles, and frequent in *Swedland* and other places nearer the pole, M. *Burrman* often and diligently observ'd. He had an opportunity, when travelling in the night time, of more accurately observing an uncommon and very singular *aurora borealis*, on *September* 20.1717, which gave him occasion to submit a conjecture about the optical nature of the phænomenon, to the consideration of the learned.

The tracks of these *chasmata* were more numerous than usual, *viz.* four or at least three whitish arches, brighter than the rest of the sky (which was clear and calm) divided by small intervals of darkness, and placed one over the other. What was a very uncommon, and at the same time an agreeable sight was, that the distances of the said arches, greatest under the *Bears* (below which the highest was not above 26 degrees, *Dubbè* a star of *Ursa Major* at times shining thro' it) gradually decreas'd on both hands towards the horizon, till they at length intersected each other in the very points of east and west, just as the meridians on an artificial globe converge towards the poles of the equator. These white arches or semicircles for the most part consisted of distinct *striæ*, perpendicular to the horizon, especially in the vertex, or under the north; a fainter and more obscure light was observ'd on the sides.

The *striæ* had a double motion, the one horizontal, the other perpendicular; the latter was much less and slower; so as scarce to disturb the circular form of the arches; the former was various, and at the same time very swift, from east to west, and back again. And as often as several *striæ* (which frequently happened) coming from different parts, met each other, whether in the middle of an arch or elsewhere, a very agreeable variety of colours was observed to proceed from this commixture or various intersection of the rays, and that in the same order, as the colours are produced in the prism, the feathers of smaller birds, or such like bodies, expos'd to the sun.

Hence

Hence we may take occaſion of farther conſidering the optical reaſons of the phænomenon, with *Des Cartes cap.* 7. *parag.* 18. *de Meteoris:* But yet M. *Burrman* does not think, that an accenſion of a more ſubtile ſulphureous matter in the inferior region of the atmoſphere is altogether to be denied a place in this matter. For, he himſelf obſerved a much greater variety of colours, with a hiſſing noiſe, like the flame of a fire, at ſeveral other times, but eſpecially in the *Chaſma* of *Mar.* 17, 1716. which was more remarkable in *Swedeland*, for a whole night together, than in *England*, *France*, *Germany* or any where elſe. We muſt, perhaps, ſuppoſe two different kinds of *aurora borealis*; the one a fiery meteor, from exhalations and effluvia; the other merely optical from the different refraction and reflexion of the ſolar rays, either in ſome icy *lamellæ*, or icicles, poſſeſſing the higher regions of the atmoſphere, or ariſing from ſome of the northern ſeas, and thence communicated to us from the clouds. Indeed, the latter ſeems to be illuſtrated by a particular experiment, invented and tried on occaſion of the already deſcribed phænomenon (yet ſomething reſembling this M. *Burrman* likewiſe obſerved in *February* 1716, at nine o'clock in the evening, conſiſting of two ſuch ſemicircular arches, but with fewer *ſtriæ* and higher above the northern part of the horizon) and is, as follows.

If a plate of tin be taken of any length and breadth, and at one ſtroke ſcraped lengthwiſe with a ſharp and ſtrong knife, till it become full of *ſtriæ*; and held in the hand in ſuch a poſition, that its plane may form equal angles with a lighted candle, and a dark wall or table; and then be bent and incurvated different ways, at one time turning its concave ſide and at another its convex ſide to the wall or table, faſter or ſlower at pleaſure; the appearances will be very like the abovementioned, and afford no leſs agreeable a ſight.

But it may happen that ſometimes, both theſe kinds of light exiſt together, and coincide with each other, ſo as that neither is the cauſe or effect of the other; but both concur to enlighten the night and ſtrike terror into the beholders. For, as M. *Burrman* has very frequently obſerved this horizontal light, *viz.* flaming zones, at one time ſingle (nay, paſſing almoſt over the zenith, which the common people, on account of the reſemblance, uſually confound with the *via lactea*, as being but a little broader) and at another time attended with columns, inverted pyramids and a variety of other figures, but without any accenſion of the air; ſo this latter frequently happens without theſe appear-

ance

ances diſtinctly obſerved, either preceeding or accompanying it: Tho' it be likewiſe true, that from ſome quarter of the heavens, at firſt only flaming, there at length dart up to the zenith and ſometimes over all the hemiſphere very ſtrong lights, either by the reflection of the rays of the ſun in the icy particles of the ocean, or in ſome cauſtic mirrors, or accended ſome other way.

But ſince no one can with certainty diſcover the true cauſes of this ſurpriſing phænomenon, previous to a due compariſon of the ſeveral obſervations taken at the ſame time in different places of the earth; it may more eſpecially appear, whether that light be ſeen under the ſame angle of altitude even in diſtant places; and whether what in one place is parallel to the horizon, be not in another perpendicular thereto; in ſhort, whether it be one and the ſame arch that is obſerved in different places; or whether here, as in the rainbow, there are not as many different arches as different ſpectators: M. *Burrman* therefore earneſtly intreats all, who have any regard for the advancement of natural knowledge, but more eſpecially thoſe in the more northerly parts of the world, carefully to obſerve this phænomenon, as to all its circumſtances, and communicate their obſervations either to the public or to him, which he thinks would be a very ſingular favour done the learned world.

A New Barometer; *by M.* Fahrenheit. Phil. Tranſ. N° 385. p. 179. *Tranſlated from the* Latin.

TO the cylinder A B (repreſented Fig. 12. Plate XII.) is joined the tube B C, to which an oblong ball C D is added, and to this the tube D E of a very fine bore. The cylinder, which can bear the heat of boiling water, is filled with a certain liquor; the ſenſible degrees of heat in the air are meaſured in the tube B C by means of the ſcale *b c*. If this thermometer be put into boiling water, the liquor therein will not only fill the ball C D, but likewiſe riſe to the different limits of the tube D E, according to the degree of heat, which the water, at the time of making the experiment, will acquire from the gravity of the atmoſphere. So that, for inſtance, if, at the time of making the experiment, the height of the mercury in the barometer be 28 *London* inches, the liquor in this thermometer will reach the loweſt place in the tube D E: But if the gravity of the atmoſphere be equal to the height of 31 inches of mercury, the liquor will be raiſed by the heat of the boiling water to the higheſt part of the tube D E; by means of the annexed ſcale *d e*, the different limits of the heat of the boiling water,

water will be denoted, not by degrees, but by the number of inches, by which the height of the mercury in barometers is commonly measured.

An Eclipse of the Moon, Nov. 1, 1724. N. S. *by F.* Carbone *and* Dominico Capasso. Phil. Trans. N° 385. p. 180. *Translated from the* Latin.

F. *Carbone* and *Dominico Capasso* observed this eclipse with two telescopes, the one eight *Paris* feet in length, and very clear, the other ten, but not so clear; yet the lunar *maculæ* were very distinctly discerned thro' both: To measure the time, they made use of a pendulum clock, pretty accurate, fixed several days beforehand at the place of observation, and by repeated trials reduced as near as possible to the mean motion of the sun, by a meridian line, they had drawn there some time before, and several times examined. The very night of the eclipse, the clock was thrice examined, in order to find its difference from the true time; first, by a transit of *Fomahant* over the meridian, at $8^h\ 17'\ 18''$; and then by a transit of *Rigel* or the foot of *Orion* at $2^h\ 35'\ 21''$; and lastly, by a transit of *Sirius* at $4^h\ 7'\ 40''$. The *R. Ascen.* were taken from *De la Hire*'s tables. They found the clock go only seven minutes too slow, which are added to the time of the following observation.

From sun-set to midnight the sky was overcast with clouds and rain; but a wind about one o'clock rendered it pretty clear, and it continued so till near three o'clock.

True time			
h.	′	″	
1	38	00	The *penumbra* begins to be sensible.
1	41	00	It becomes thicker.
1	43	29	It becomes very thick.
1	47	45	The shadow begins.
1	49	25	The disk of the moon appears eclipsed.
2	0	16	The shadow at *Aristarchus*.
2	0	39	At *Plato*.
2	1	10	*Aristarchus* entirely immerged.
2	6	22	*Archytas*.
2	8	7	*Aristoteles*.
2	10	29	*Pitheas*.
2	11	28	*Galilæus*.

True

True time h.	′	″	
2	13	22	The ſhadow at the eaſtern ſhore of *Mare ſerenitatis*.
2	15	34	*Endymion* entirely immerged.
2	18	2	*Copernicus* begins to immerge.
2	20	7	Entirely immerged.
2	21	5	*Poſſidonius* begins to immerge.
2	22	8	Entirely immerged.
2	27	49	*Ricciolus* begins to immerge.
2	31	56	The ſhadow at *Grimaldus*.
2	34	37	At the north ſhore of *Mare Criſium*.
2	37	17	*Proclus* immerges.
2	40	0	A cloud comes on, that quite covers the moon and would continue for ſome time.
3	25	0	The cloud goes off; and now *Grimaldus*, *Ricciolus*, *Keplerus* and *Galilæus* emerged out of the ſhadow.
3	29	2	*Ariſtarchus* emerges.
3	30	30	*Copernicus* begins to emerge.
3	31	34	Entirely emerged.
3	39	18	*Pitheas* emerges.
3	47	46	*Timocharis*.
3	54	57	*Archimedes*.
3	57	18	*Plato* begins to emerge.
3	58	59	Entirely emerged.
4	2	0	The moon again overcaſt with a ſmall cloud.
4	6	0	Now ſhe appears again.
4	8	15	*Ariſtoteles* entirely emerged.
			The moon at times overcaſt with ſmall clouds, the emerſions of the other *maculæ* could not be exactly obſerved.
4	20	36	The end of the eclipſe, obſerved, probably, later by reaſon of a thin vapour that interpos'd.
4	26	0	The end of the thicker *penumbra*.
4	28	0	The end of the ſenſible *penumbra*.

S. *Carbone* and *Capaſſo* endeavoured with all poſſible accuracy to obſerve this eclipſe; being furniſhed by the king of *Portugal* with a great variety of inſtruments.

Eclipses of Jupiter's *first* Satellite, *observed at* Lisbon; *by* F. Carbone. Phil. Transf. N° 385. p. 185. *Translated from the* Latin.

1723	h.	′	″
Emersions			
July 23.	7	47	0
September 7.	8	21	48
1724			
Immersions			
June 8. *Mane*	2	3	28
15.	3	56	27
30.	2	8	51
Emersions			
September 2.	9	36	57
9.	11	34	26
25.	9	59	21
October 4.	6	26	44
18.	10	21	20
November 3.	8	42	30

The preceeding observations were made with S. *Campani's* telescope, 30 *Roman* palms in length.

An Eclipse of the Moon *at* Paris, Nov. 1. 1724. N. S. *by M.* Maraldi. Phil. Transf. N° 385. p. 186. *Translated from the* French.

True time.			
h.	′	″	
2	33	30	The beginning of the eclipse.
2	46	15	The shadow at *Aristarchus*.
2	56	20	At *Galilæus*.
3	20	30	At the north shore of *Mare Caspium*.
3	23	30	At *Proclus*.
4	14	30	*Aristarchus* emerges.
4	17	50	*Copernicus* entirely emerged.
4	33	34	*Timocharis* emerges.
4	44	23	*Plato* entirely emerged.
5	6	30	The end of the eclipse.
2	33	0	The duration of the eclipse.

The

The Longitude *of the* Fort *of* New-York, *determined from Eclipses of the first Satellite of* Jupiter; *together with the* Variation *of the* Magnetic Needle; *by Mr.* William Burnet. Phil. Transf. N° 385. p. 162.

MR. *Burnet* formerly determined the Lat. of the fort of *New-York* to be 40° 40′.

August 9. 1723. At *London* the time of the emersion of *Jupiter*'s first satellite, according to Mr. *Pound*'s tables, reduced to apparent time

	h	′	″
reduced to apparent time	16	9	25
Time, as observ'd at *New-York*	11	10	43
Difference of meridians	4	58	42

Aug. 25. Alt. of the sun's superior limb.	°	′	″	Time by the clock. h.	′	″	Time by calculation. h.	′	″
Sun's declina.	49	30	00	10	17	52	10	17	28
6° 55′	51	13	30	10	33	10	10	32	8
August 26.									
Sun's declina.	46	24	0	9	57	40	9	56	25
6° 33′	47	50	0	10	8	22	10	6	57

	h	′	″
Time of emersion by Mr. *Pound*'s tables	14	31	25
Equation of time to be added.	00	1	22
	14	32	47
Time observ'd by the clock	9	35	14
The same corrected	9	34	14
The difference of meridians	4	58	33

This Mr. *Burnet* takes to be the distinctest and best observation.

September 10. Alt. of the sun's superior limb.	°	′	Time by the clock. h.	′	″	Time by calculation. h.	′	″
Sun's declina.	33	21	9	1	0	9	0	16
49′	34	6	9	6	1	9	4	49
September 17.								
Sun's declina.	17	17	4	21	40	4	21	44
1° 54′	15	15	4	33	5	4	32	47

	h.	′	″
September. 10.			
Time of emersion by the clock	8	10	0
Time of emersion by Mr. *Pound*'s tables	12	50	36
Equation of time to be added	0	6	54
	12	57	30
Corrected time at *New-York*	7	59	8
Difference of meridians	4	58	22

June 26.	Altitude of the sun's superior limb.		Time by the clock.			Time by calculation.		
June 20.	°	′	h.	′	″	h.	′	″
Sun's declina.	56	44	9	48	3	9	43	37
23° 7′	60	27	10	9	40	10	5	5
June 27.								
Sun's declina.	63	31	10	27	43	10	27	5
22° 26′	65	21	10	40	0	10	39	27

	h.	′	″
June 26. Time of immersion by the clock	11	41	12
Time of immersion by Mr. *Pound*'s tables	16	43	2
Equation of time to be substracted	0	4	26
	16	38	36
Time at *New-York* corrected	11	40	15
Difference of meridians	4	58	21

The mean of all these observations is $4^h\ 58'\ 30''$, which to $3''$ agrees with that observation, Mr. *Burnet* took to be the most exact; and therefore, the Long. of *New-York* is nearly $74°\ 57'\ 30''$ west from *London*.

The variation of the magnetic needle was observ'd in 1724, to be 7° 20′ west. Mr. *Wells* surveyor general of *New-York* observ'd it in 1686 to be 8° 45′; by which it appears to decrease about 1° 25′ in 38 years, or a little more than 2 minutes in a year.

The Longitude *of* Lisbon, Paris *and* London, *determined from the* Eclipses *of the* Moon, Nov. 1. 1724. *and* Jupiter's *Satellites; by F.* Carbone. Phil. Trans. N° 385. p. 186. *Translated from the* Latin *and* French.

IT is to be observ'd, that the difference between the meridians of *Lisbon* and *Paris*, was now found less than what F. *Carbone* imagined; not by new observations at *Lisbon*, but by comparing these already made there with those at *Paris*; having receiv'd none before, by which he could determine with certainty the said difference; but he only compar'd his own with the computations of M. *Lieutaud*, calculated to the meridian of *Paris*, as deliver'd in his treatise, entituled *Connoissance des Temps*, and publish'd yearly by order of the Royal Academy. F. *Carbone* found them too wide of the truth, especially as to the immersions and emersions of *Jupiter's* innermost satellite: For, the observations made in the Royal Observatory, differ from these sometimes 2, sometimes 3, and at other times even 4 minutes.

F. *Carbone* compar'd his own observation of the lunar eclipse at *Lisbon*, with that made at *Paris* by M. *Maraldi*; to which he subjoins the comparison of the immersions and emersions, observ'd at both places.

Tho' in the eclipse of the moon, *Nov.* 1, 1724, the shadow was not well defin'd either at *Lisbon* or *Paris* (which render'd the determining these phases the more difficult) however, the greatest part of the observations agrees so well together, that it was thought proper to compare the principal phases, which seem to have been observ'd with the greatest accuracy, in order to determine the difference of the meridians of *Lisbon* and *Paris*.

h.	′	″	
1	47	45	The beginning of the eclipse at *Lisbon*.
2	33	30	The beginning of the eclipse at *Paris*.
	45	45	The difference of the meridians of *Lisbon* and *Paris*.
2	0	16	At *Lisbon* the shadow at *Aristarchus*.
2	46	15	At *Paris*.
	44	52	The difference.
2	11	28	At *Lisbon* the shadow at *Galilæus*.
2	56	20	At *Paris*.

44 51

h.	′	″	
	44	52	The difference.
2	34	37	At *Lisbon* the ſhadow at the north ſhore of *Mare Caſpium*.
3	20	30	At *Paris*.
	45	53	The difference.
2	37	17	At *Lisbon* the ſhadow at *Proclus*.
3	23	30	At *Paris*.
	46	13	The difference.
3	29	2	At *Lisbon Ariſtarchus* emerges.
4	14	30	At *Paris*.
	45	28	The difference.
3	31	34	At *Lisbon Copernicus* entirely emerged.
4	17	34	At *Paris*.
	46	16	The difference.
3	47	46	At *Lisbon Timocharis* emerged.
4	33	34	At *Paris*.
	45	48	The difference.
3	58	59	At *Lisbon Plato* entirely emerged.
4	44	23	At *Paris*.
	45	24	The difference.
4	20	36	The end of the eclipſe at *Lisbon*.
1	6	30	At *Paris*.
	45	54	The difference.

According to theſe obſervations, the duration of the eclipſe at *Lisbon* was 2h. 32′ 51″, only 9 ſeconds leſs than it was obſerv'd at *Paris*; and the difference of meridians, that reſults from the obſervations of the beginning and end of the eclipſe, is 45′ 50″; which comes very near what reſults from the compariſon of the other *maculæ*, obſerv'd at *Lisbon* and *Paris*.

At the Royal Obſervatory at *Paris* M. *Maraldi* made ſeveral obſervations on the eclipſes of *Jupiter*'s ſatellites, correſponding with thoſe ſent him from *Lisbon*; the compariſon is as follows.

		h.	′	″	
June	30. 1724	2	8	51	The immerſion at *Lisbon*.
		2	54	41	At *Paris*.
			45	50	The difference.
Sept.	2.	9	36	57	The emerſion at *Lisbon*.
		10	22	46	At *Paris*.
			45	49	The difference.

September

	h.	′	″	
September 25.	9	59	21	The emerſion at *Lisbon*.
	10	45	5	At *Paris*.
		45	44	The difference.
October 4.	6	26	44	The emerſion at *Lisbon*.
	7	11	58	At *Paris*.
		45	14	The difference.

The greateſt part of theſe obſervations agrees in making the difference of meridians between *Lisbon* and *Paris* 45′ 48″ of time, which agrees with all poſſible accuracy with that determined by the laſt obſervation of the eclipſe of the moon.

If the aforeſaid difference be true, *viz.* 45′ 48″, the difference between the meridians of *Lisbon* and *London*, will be 36′ 7″

An excretory Duct *from the* Glandula renalis. Phil. Tranſ. N° 385. p. 190.

THE celebrated anatomiſt S. *Valſalva*, already known by his noble treatiſe *de aure humanâ*, has made a conſiderable diſcovery: He has found the excretory ducts of the *glandulæ renales* or *renes ſuccenturiati*, which diſcharge themſelves into the parts of generation; that is, into the *epididymides* in men and into the *ovaria* in women. He has read a learned diſſertation, relating to this diſcovery, before the academy of ſciences in *Bologna*, in which he undertakes to prove, that thoſe *renes ſuccenturiati* are to be reckoned among the principal organs of generation.

An Account of the Currents *at the* Streights Mouth. Phil. Tranſ. N° 385. p. 191.

CAPE *Spartel* and *Cape Trafalgar* from the weſtern ocean are known to form the ſtreights mouth; whence a current, in the middle of the channel (which is about five leagues broad) betwixt the *Barbary* and *Spaniſh* land, runs at leaſt two miles each hour, as far as *Ceuta* point, and there the two coaſts opening about 18 leagues diſtance from each other, the current does not run above one mile an hour, and ſo continues as far as *Cape de Gat*, which is 70 leagues up the *Mediterranean*. Our mariners obſerve a current to ſet to the weſtern ſea or the great ocean from *Ceuta*, along the *Barbary* ſhore, and from *Gibralter* along the *Spaniſh* ſhore; but that on the *Barbary* ſhore is generally their common rout, not only as being the freeſt from rocks and leſs dangerous; but becauſe the tide is much ſtronger than

than it is on the other ſide, which the ſooner helps the ſhips out of the ſtreights, which are narroweſt between the points of *Gibralter* and *Ceuta*; at which laſt place, a neck of land extends itſelf a conſiderable way into the ſea; and it is the opinion of the author and others, that whereas the current runs, as aforeſaid, two miles an hour againſt this neck of land, the water there meets with ſo violent an oppoſition in its courſe, as occaſions it to rebound with ſo much force, that part of it returns back along the ſame coaſt, and ſo out of the *Streights* mouth; which, with the ſmall tide that ſets out on the *Spaniſh* ſhore, it is believed, may exhauſt a conſiderable part of that current; which continually ſets in to the eaſtward at the rate already mentioned. What the author takes to be very remarkable is, that in 1712 M. *de l'Aigle* commander of a *French* privateer, called the Phœnix of *Marſeilles*, giving chaſe near *Ceuta* point to a *Dutch* ſhip bound for *Holland*, came up with her in the middle of the gut or *Streights*, between *Tariffa* and *Tangier* and there gave her one broadſide, which directly ſunk her; and a few days after, the ſunk ſhip with her cargo of brandy and oil, aroſe on the ſhore near *Tangier*, which is, at leaſt four leagues to the weſtward of the place where ſhe ſunk, and directly againſt the ſtrength of the current; which made many apt to think, there is a recurrency in the deep water in the middle of the gut, that ſets outwards to the grand ocean, which, the author thinks, this accident very much demonſtrates; and poſſibly a great part of the water which runs into the *Streights*, returns that way and along the two coaſts already mentioned; otherwiſe, this ſhip muſt of courſe have been driven towards *Ceuta* and ſo upwards. The water in the gut muſt be very deep, ſeveral of the commanders of our ſhips of war having attempted to ſound it with the longeſt lines they could contrive; but could never find any bottom.

Ambergris *found in* Whales; *by Dr.* Boylſton. Phil. Tranſ. N° 385. p. 193.

THE moſt learned part of mankind are ſtill at a loſs about ſeveral things, even in medical uſe; and particularly ſo, in what is called *ambergris*, till our whale fiſhermen of *Nantucket* in *New England* lately made the diſcovery.

Upon cutting up a *Sperma ceti* bull whale, they accidentally found in him about 20 pound weight of that drug. After which, they and other fiſhermen became very curious in ſearching all ſuch whales they killed; and it has ſince been found in ſmaller

ſmaller quantities, in ſeveral male whales of that kind and in no other, and that ſcarcely in one of a hundred of them. They add farther, that it is contained in a *cyſtis* or bag, without any inlet or outlet thereto; and that they have ſometimes found the bag empty and yet entire.

The bag is no where to be found, but near the genital parts of the fiſh. The *ambergris* is, when firſt taken out, moiſt, and of an exceeding ſtrong and offenſive ſmell.

Obſervations on ſome of the Plants *in* New England, *with remarkable inſtances of the Nature and Power of* Vegetation; *by Mr.* Paul Dudley. Phil. Tranſ. N° 385. p. 194.

THE plants of *England*, as well thoſe of the fields and orchards, as of the garden, that have been brought over into *New England*, ſuit very well with the ſoil and grow to great perfection.

Their apples are, without doubt, as good as thoſe of *England* and much fairer to look to, and ſo are the pears; but they have not got of all the ſorts.

Their peaches do rather excel thoſe of *England*, and then they have not the trouble or expence of walls for them; for, their peach-trees are all ſtandards; and Mr. *Dudley* has had in his own garden 7 or 800 fine peaches of the *rare-ripes*, growing at a time on one tree.

The people of late years have run ſo much upon orchards, that in a village near *Boſton*, conſiſting of about 40 families, they made in the year 1721 near 3000 barrels of cyder; and in another town of 200 families, in the ſame year, Mr. *Dudley* was credibly informed, they made near 10,000 barrels. Some of their apple-trees will make 6, ſome have made 7 barrels of cyder; but this is not common; and the apples will yield from 7 to 9 buſhels for a barrel of cyder. A good apple-tree in *New England* will meaſure from 6 to 10 foot in girt. Mr. *Dudley* has ſeen a fine pear-main tree, at a foot from the ground, meaſure 10 feet and four inches round. This tree, in one year, bore 38 meaſured buſhels of as fine pear-mains, as ever he ſaw in *England*. A *Kentiſh* pippin at 3 foot from the ground, 7 foot round, and a golden roſſetin 6 foot round. The largeſt apple-tree he could find was 10 foot 6 inches round; but this was no graft.

An *orange* pear-tree grows the largeſt, and yields the faireſt fruit: He obſerved one of them near 40 foot high, that meaſured 6 foot and 6 inches in girt, a yard from the ground, and

bore 30 bushels at a time; and in the year 1724 he measure
an *orange* pear-tree, 11 inches round the bulge. He had a pear-
tree, that measured 5 foot 6 inches round. One of Mr. *Dud-
ley*'s neighbours had a *Bergamot* pear-tree, brought from *Eng-
land* in a box, about the year 1643, that measured 6 foot round
and bore 22 bushels of fine pears in one year. About 20 years
before, the owner took a cyon and grafted it upon a commo
hedge-pear; but the fruit did not prove altogether so good
and the rind or skin was thicker than that of the original.

Their peach-trees are large and fruitful, and commonly be
in three years from the stone. Mr. *Dudley* had one in his gar-
den of 12 years growth, that measured two foot and one inc
round, a yard from the ground, which two years before, bo
near a bushel of fine peaches. Their common cherries are n
so good as the *Kentish* cherries of *England*, and they have n
dukes or heart cherries, but in two or three gardens.

Some years before, Mr. *Dudley* measured a *Platanus occiden-
talis* or *button-wood* tree, as they are called, that was nine yar
round and it held its bigness a great way up. This tree, wh
cut down, Mr. *Dudley* was informed, made 22 cord of woo
A gentleman told him, that in the forest he observed a streig
ash, that grew like a pillar, of a considerable height and f
from limbs, that measured 14 foot 8 inches round, near a ya
from the ground; and Mr. *Dudley* saw a sassafras tree, th
measured five foot three inches round. Among their trees
quick and easy growth, the abovementioned *button-wood* a
the *locust* tree, are the most remarkable: As to the latter, a
cording to the description Mr. *Moore*, while in *New Engla*
gave Mr. *Dudley* of the *manna* tree, their *locust* tree may
called the *American manna*. A seed of it, blown off from
tree into Mr. *Dudley*'s garden, took root of itself; and in
than two years was got above six foot high, and as big about,
a common walking cane. He several times propagated
platanus, by cutting off sticks of five or six foot long,
setting them a foot deep in the ground in the spring of
year, when the season is wet; they thrive best in a moist so

An onion, set out for seed, will rise to four foot nine inc
in height; a parsnip to eight foot; red orrice to nine foot
white orrice to eight. In the pasture-grounds Mr. *Dudley* m
sured seed mullen nine foot two inches in height; and one of
common thistles upwards of eight foot.

Among the remarkable instances of the power of vegetat
Mr. *Dudley* had a well attested account from Mr. *Edward*

Win

Windsor of a pompion seed, as follows; that in *1699* a single seed was accidentally dropped in a small pasture, where cattle had been foddered for some time. This single seed took root of itself, and without any manner of care the vine run along over several fences, and spread over a large piece of ground far and wide, and continued its progress till the frost came and killed it: This seed had no more than one stalk, but a very large one; for, it measured eight inches round: From this single vine they gathered *260* pompions; and one with another, as big as a half peck; enough in the whole to fill a large tumbrel, besides a considerable number of small and unripe pompions, they made no account of. There is an account in the *Philosophical Transactions* of a single plant of barley, that by steeping and watering it with salt-petre, dissolved in water, produced 249 stalks and 18,000 grains; but then there was art and even force in that case: Whereas, in this, there was nothing but pure nature and accident.

The *Indian* corn is the most prolific grain they have, and commonly produces 1200, and often 2000 grains from one single grain; but the fairest computation is thus; six quarts of this grain will plant an acre of ground; and it is not unusual for an acre of good ground to produce 50 bushels of corn.

Indian corn is of several colours, as blue, white, red and yellow; and if they are planted separately, so that no other sort be near them, they will retain their own colour. But if in the same field you plant the blue corn in one row of hills (as they are called) and the white or yellow in the next row, they will mix and interchange colours; that is, some of the ears of corn, in the blue corn rows, shall be white or yellow; and some again, in the white or yellow rows, shall be blue. The hills of *Indian* corn are generally about four foot asunder and so continued in a streight line, as far as the field will allow; and then a second line or row of hills, and so on; and yet this mixing and interchanging of colours has been observed, when the distance between the rows of hills has been several yards: And Mr. *Mayhew* of *Martha's Vineyard*, an island in the province of *New England*, assured Mr. *Dudley*, that the blue corn has thus communicated or interchanged, even at the distance of four or five rods; and particularly in one place, where there was a broad ditch of water between them. Some of our people, but especially the *Aborigines*, have been of opinion, that this commixture and interchange of colours, was owing to the roots and small fibres, reaching to and communicating with

each other; but this must certainly be a mistake, considering the great distance; especially at some times and a-cross a canal of water; for, the smallest fibres of the roots of the *Indian* corn cannot extend above four or five foot: Mr. *Dudley* is, therefore, of opinion, that the *stamina* or principles of this surprising copulation or commixture of colours are conveyed by the wind; and that the season of it is, when the corn is in the earing, and while the milk is in the grain: For, at that time, the corn is in a sort of estuation and emits a strong scent. One thing, which confirms the air being the medium of this communication of colours in the corn, is an observation of one of Mr. *Dudley*'s neighbours, *viz.* that a close, high board fence, between two fields of corn, that were of a different colour, entirely prevented any mixture or alteration of colour, from that they were planted with.

An apple-tree in the town, belonging to Mr. *Dudley*, bears a considerable quantity of apples; especially every other year, tho' it never had a blossom: For three years, he went in the proper season and observed it; and when all the rest of the orchard was in the bloom, this tree had not one single blossom: Not contented with once going, he went again and again, till he found the young apples perfectly formed. In 1723 he went early, not knowing but it might blow sooner than the other trees; but he found no blossoms; and the owner with several of his neighbours, assured Mr. *Dudley* they had known the tree upwards of 40 years, and that it never had any blossom. Mr. *Dudley* opened several of the apples and observed but very few seeds in them; and some of them lodged single in the side of the apple. This tree was no graft, and the fruit but ordinary for taste: He could not perceive, by his observation, but that in all other respects, it fructified like other apple-trees.

A Dissertation on the Figure of the Earth; *by Dr.* Desaguliers. Phil. Transf. N° 386. p. 201.

THAT the earth is of a spherical figure, or nearly such, has been proved so often and by so many unanswerable arguments, that it would be tedious to repeat them. But as a little variation from a true sphere (besides the irregularity of high hills and deep valleys) does not hinder us from calling the earth a globe; so to determine what that variation may be, since modern philosophers are divided about it, may not be an ungrateful subject.

M. *Cassini*

M. *Cassini* affirms 'that the earth is an oblong spheroid,
'higher at the poles than at the equator, making the axis
'longer than the equatorial diameter about 13 *French* leagues;
'which he deduces from comparing his father's measures of the
'meridian from *Paris* to the *Pyrenæan* mountains, with those
'of M. *Picard*; of which an account may be seen in the *Memoirs of the Royal Academy* for the year 1713. But having
'afterwards continued the meridian, which is drawn thro'
'*France*, from *Paris* to *Dunkerque*, he still draws consequences to prove the earth an oblong spheroid; but then he
'makes the axis exceed the equatorial diameter 34 leagues.

'Sir *Isaac Newton* makes the earth higher at the equator;
'and consequently, flatted towards the poles, reckoning its
'equatorial diameter 34 *English* miles longer than the axis;
'which he proves from the principles of gravity, and the centrifugal force, that arises from the diurnal rotation of the
'earth; and to confirm this he mentions several experiments on
'pendulums, which have been made shorter near the equator,
'than in greater latitudes, to swing seconds.

These are the two opinions which have divided philosophers, and which Dr. *Desaguliers* proposes to examine.

M. *Cassini* taking the measures abovementioned to be exact enough, not only to determine the magnitude of a degree of the earth, corresponding with a degree of a great circle of the heavens; but also to shew the difference in the degrees of the earth (reckoning those measured in the south of *France* to exceed those towards the north, by a certain number of toises and feet) demonstrates, that if the degrees of the earth are longer towards the equator than the poles, the plane of the meridian must be an ellipse, whose long axis is that of the earth. Here follows his first demonstration; vide the *French Memoirs* for the year 1713.

'Let BDCR (Fig. 13. Plate XII.) be an ellipsis representing
'a meridian of the earth, whose poles B and C are at the extremities of the great axis B C, and whose *foci* E and F are
'taken at pleasure. Now to divide this ellipse into degrees,
'that is, to find several points H I V, such, that the distance
'of every one of them, from the pole to the zenith, shall be of
'any given number of degrees.

'*Demonstration.* From E one of the *foci* of the ellipsis,
'draw the line E T; so that it may form, with the axis B C,
'the angle B E T, equal to the distance given from the pole to
'the

the zenith: From the other *focus* F, with the diſtance BC, equal to the axis, draw an arch to cut the line E T at T; I ſay, that the line F T, drawn from the point T to the *focus* F, will cut the ellipſis at the point H; which point is ſuch, that the diſtance of the pole, from its zenith, contains the given number of degrees.

'From the point H, raiſe HZ, perpendicular to the ellipſis, which will paſs thro' the zenith Z; and being produced inwards, will meet the axis of the earth at O; and (by the property of the ellipſis) divide the angle EHF into two equal parts: From the point H, likewiſe draw HP, parallel to the axis BC, and directed to the pole P, ſuppoſed at an infinite diſtance: The angle PHZ or POZ, meaſures the diſtance from the pole to the zenith, of an inhabitant dwelling upon the earth at the point H. FT is equal to the axis BC, by conſtruction; but by the property of the ellipſis, BC is equal to EH plus HF, taking away from both FH, which is common, EH will remain equal to HT; the angles ETH, TEH, will, therefore, be equal, and conſequently, each of them will be half of the external angle EHF: But the angle EHO is likewiſe equal half the angle EHF; therefore, the angles TEH, EHO, will be equal to each other; and conſequently, the lines ET and HO will be parallel to each other; and the angle POZ, which meaſures the diſtance, from the pole to the zenith of the point H, will be equal to the angle BET, which was, by conſtruction, taken equal to the given diſtance of the pole from the zenith; Q. E. D.

'Now, if the proportion of the longeſt diameter of the ellipſis BC to EF, the diſtance of the *foci*, be taken at pleaſure, one may, by calculation, find all the points of the ellipſis as H, to determine the degrees by this analogy as FT or BC: is to EF :: So is the ſine of the angle PET (the given diſtance from the pole to the zenith): To the ſine of the angle ETF, or TEH; whoſe quantity will conſequently be known. This angle TEH being added to the angle PEF, the given diſtance, from the pole to the zenith, of the point H, will give the quantity of the angle BEH, which a line drawn from the *focus* to H, the point required, makes with the axis of the ellipſis.

'Then, in the triangle EHF, whoſe ſide EF is known, as well as the angle EHF, which is double the angle TEH,

'and

'and the angle F E H ſupplement of the angle B E H; one
'ſhall have the length of the ſide E H, known in parts of the
'axis B C.

'After the ſame manner may be found the angles B E I,
'B E V, &c. and the length of the lines E I, E V, to determine the diſtance, from the pole to the zenith, of all the
'degrees of the circumference of the earth; and in the rectilinear triangles H E I, I E V, whoſe ſides H E, E I, E V,
'are known, as well as the angles, comprehended between
'the ſides H E, E I, E V, which are the differences of the
'angles B E H, B E I, B E V, determined above; one ſhall
'find the length of the chords H I, I V, comprehended between each degree.'

M. *Caſſini* in the *Memoirs* for the year 1718, repeats the ſame demonſtration; only that, before it, he ſhews, that if ſeveral points be taken upon a terreſtrial meridian, on the ſurface of an elliptic earth, as G H I K, (Fig. 14.) in ſuch manner that their reſpective zeniths Z L M N, are diſtant from each other, an equal number of degrees, meaſured in a celeſtial meridian; the lines Z G, L H, M I, N K (which are perpendicular to the ellipſis) being produced, will meet in the points O, R and S, forming equal angles: But as thoſe angular points are not equally diſtant from the curve of the ellipſis, that elliptic arch muſt be the longeſt, whoſe angular point is fartheſt off. Now, by the former demonſtrations it appears, that thoſe arches, which are taken neareſt to the leſſer axis, will have their angular points farther removed, &c.

If M. *Caſſini's* meaſures of terreſtrial degrees, decreaſing from the equator towards the pole, were grounded on obſervations liable to no error, he would have fully proved his figure of the earth: But ſince thoſe meaſures (however accurately taken) are not built upon a mathematical certainty, his premiſes may be called in queſtion; and his concluſion, tho' mathematically drawn from theſe premiſes, is only probable.

Now therefore, if Dr. *Deſaguliers* can ſhew from undoubted phænomena, that his concluſion will lead to an abſurdity, his meaſures muſt be falſe; tho' his reaſoning from them be juſt. This the Dr. endeavours to do firſt, which will diſprove his figure of the earth; and afterwards to point out ſome of the errors, which he ſuppoſes to have occaſion'd the miſtake in the meaſures.

M. *Caſſini*,

M. *Cassini*, as well as the *English* astronomers, believes that the earth makes one revolution about its axis, once in $23^h\ 56'$; because in that time, the plane of any meridian returns to the same fixed star, from which it had departed.

Let H be taken in any parallel of Lat. as for example, in the Lat. of $51^\circ\ 46'$, a plumb line L H, will be perpendicular to the curve B H at H, and produced pass thro' the zenith of the point H, if the earth had no diurnal rotation; but since the earth moves round its axis, all bodies upon its surface, endeavour to fly from the axis of their motion with a force proportionable to their distance from it, in a direction along the plane of that parallel, in which they are. Let that force (explained by M. *Huygens* and called a centrifugal force) be represented by the line H *l*, or its equal and parallel L *h*; now a plummet placed at L, if the earth stood still, would descend in the line L H; but as it is at the same time acted upon by the force H *l* in the direction L *h*, it will move in the direction L *l*, diagonal of the parallelogram H *l*, according to the known laws of mechanics; and the plumb line L H, instead of being perpendicular to the curve at H, will in Lat. $51^\circ\ 46'$ form an angle of $5'$ with H L. This angle will be less towards the poles, till it quite vanish at the pole, as it also does at the equator. Now since there is no such angle observed; but in all water levels we find the plumb-line, always perpendicular to the line of level, the surface of the earth must be depressed towards G, and rise farther from the axis towards I, in order to become perpendicular (that is, to have its tangent perpendicular) to the line L *l*, in which we have shewn the plumb-line must descend.

If there be any body so fond of M. *Cassini*'s hypothesis, as to deny the diurnal motion of the earth, for the sake of it, the Dr. hopes they will be convinced, when he shews the measures, upon which it is fonnded, to be insufficient for determining the different lengths of the degrees of a terrestrial meridian.

But here the Dr. would not be thought to endeavour to lessen the commendation due to the Gentlemen of the *Royal Academy of Sciences*, for carrying on a meridian the whole length of *France*, from *Dunkerque* thro' the *Royal Observatory* at *Paris*, quite to the *Pyrenæan* mountains on the borders of *Spain*. Astronomy and geography are, doubtless, much indebted to the encouragement, given by the *French* government, and to the care of their mathematicians, who have

omitted

omitted no proper method for drawing their meridian, and correcting it, as they went on. So many observations of the rising and setting sun, so many equal altitudes of the same stars, accurately taken, so many digressions of stars, so many other observations, made with the telescope and good pendulum-clocks, &c. all compared together, for the true settling of the direct way of this famous meridian, leave no doubt, but that it is as perfect, as the nature of the thing is capable of. And certainly, by the help of this line, and the several triangles made use of for carrying it on, a better map of *France* is made, than has ever been of any country before: Nay, besides, the Dr. thinks, we may, at a medium, very well receive their number of 57,060 or 57,061 toises, for the measure of a degree of a meridian of the earth, one with another. But to say, that those gentlemen could observe the latitude so nicely, as to find a difference in the length of the terrestrial degrees, and that only of 11 or 12 toises (when they made it the least) or of 31 toises (when they made it the most) is attributing to them an exactness, so far beyond the nature of the instruments, which they made use of, that it would be rather a dispraise than commendation to insist upon it.

For, in the first place, the instrument, with which they took observations for the latitude, at the two ends of their meridian, was a ten foot sector (which was worse than that, which M. *Picard* had made use of before; because the telescope of his sector was of 10 foot; whereas M. *Cassini*'s was but of three foot, tho' applied to the 10 foot sector) where the two hundredth part of an inch answers to 8 seconds of a degree. Now the two hundredth part of an inch, being one of the least visible parts, that we can see in a divided line, they could not take an angle nearer than that; nay, their instrument, according to their own description of it, was divided but to every 20 seconds. Now they allow, that 16 toises upon the surface of the earth, answer to 1 second in the heavens; and they do not pretend to have taken an observation nearer than to about 3 seconds; which, therefore, cannot determine a difference less than 48 toises; whereas the degrees are only supposed to decrease, at most, 31 toises each, from *Collioure* to *Dunkerque*. But an error of 8 seconds would make a difference of 128 toises, on the surface of the earth, above 10 times greater than the difference of degrees in the first supposition, and 4 times greater than that diffe-

difference in the last. Besides, the Lat. was not observ'd in the intermediate places between *Paris* and *Collioure*, with the above-mentioned instrument of 10 foot radius; but they made use of a quadrant, whose radius was only 39 inches, and sometimes an octant of three foot radius: Nay, they say themselves, in their account, that it is not the observations, made at the ends of the meridian, that we are to deduce the difference of the length of a degree from; but the altitudes, taken at several places between the extremes; and if we grant, that they can take an angle very well, to four or five seconds, with the large instrument, they cannot come nearer than 12 or 15 seconds, with the quadrant or octant, which we must depend upon for the difference of the measure of degrees: So that upon the whole, we are to determine a length of 31 toises, by an instrument, which is liable to error upwards of 200 toises.

If any consequences of this kind could be drawn from actual measuring, a degree of Lat. should be measur'd at the equator, and a degree of Long. likewise; and a degree very northerly; as for example, a whole degree might be actually measur'd upon the *Baltic* sea, when frozen, in the Lat. of 60 degrees. There, according to M. *Cassini*'s last supposition, a degree would be of 56,653 toises; whereas, at the equator, it would be of 58019 toises; the difference being 1364 toises, that is, about the two and fortieth part of a degree, which must be sensible; and the degree of Long. would likewise be, according to him, of 56,87 toises, by 1202, or the forty-eight part less, than a degree of Lat. at the same place.

But here it may be objected, that tho' the Lat. was not taken with the ten foot sector, in the intermediate places between *Paris* and *Collioure*; yet the Lat. was taken with that instrument at *Dunkerque*, *Paris* and *Collioure*; and therefore, the southern part of the meridian, containing 6° 18′ 56″, may be compar'd with the northern part, which contains 2° 12′ 16″; and that the former seems to contain more toises, in proportion to the difference of Lat. at its extremities, than the latter. To this may be answered, that, even in this case, the observations made cannot be accurate enough to determine the difference of the length of degrees: But there is another error, which might considerably mislead the *French* gentlemen, and make the degrees appear longer in the south of *France*, that is, the error in taking the true height of several mountains in *Auvergne*, *Languedoc*, and among the *Pyrenees*. For, if they have allow'd too much for the

the refraction of the air (which, by the observations of travellers, is greater towards the northern regions, and diminishes as we go southward) the heights of those mountains will be taken too little, and their bases, consequently, longer, which will make the degrees appear larger than they are.

For example, let ABCD (Fig. 15.) be a mountain, as that of *Rodez* in the Lat. of 44° 21′, whose height BD is 300 toises, and whose sides AB and BC (suppos'd to form an angle of 26° 33′ with the horizon) are found by trigonometry to be of 670,8 toises each; if by a mistake, in taking the height, it be suppos'd only equal to ED or 257 toises; then the lines AB and BC will become EF and EG; so that the base AC, which before was of 1200 toises, will become equal to FG, which will appear to be 1279,6 toises (by *Eucl.* 47. 1.) Now one such mistake in one degree will give a difference above twice as great as the suppos'd difference of degrees in that Lat. which they make of 31 toises: And that there was a mistake of this kind in taking the height of that mountain, the Dr. will shew anon.

The vapours that generally float in the air about the top of high hills, make it so difficult to take their heights exactly, that experiments, made with the barometer, will, by observing the fall of the mercury, shew the height nearer than any thing else we know of. There were, it is true, several experiments made with the barometer (vide Memoirs of the Royal Academy for the year 1718. c. 10.) where the differences of the height of the mercury, from the heights at which it stood at the Royal Observatory, are said to answer to so many toises: But of nine observations mentioned by M. *Cassini*, there are not two, where the number of toises, said to correspond to the heights of the barometer, do agree together.

The first experiment of the barometer, there mentioned, made at *Collioure*, was this; 'at the height of 11 toises and ' $\frac{1}{2}$ above the sea, the barometer was set up, and the mercury ' stood three lines and $\frac{1}{3}$ higher than at the Royal Observa- ' tory (in the Tower of the eastern hall) at the same time; ' and therefore, since that tower is 44 toises higher than the ' sea, three lines and a third of mercury must answer to 32 ' toises and $\frac{1}{2}$'.

Now, reducing these toises to feet, and dividing by 3 and $\frac{1}{3}$, it will appear, that a height of 58,5 feet will answer to the fall of one line of mercury in the barometer: Let this be

taken as the ſtandard, and the other obſervations compar'd with it. This may be done by the following table, where the firſt column ſhews the place, where the obſervation was made; the ſecond, the fall or riſe of mercury at each place, expreſs'd in lines, or twelfth parts of a *French* inch; the third, the heights or depths anſwering to thoſe lines of mercury, which, in the Memoirs are given in toiſes, but here reduced to feet; the fourth, the number of feet anſwering to one line of mercury in each obſervation, which is the quotient of the feet in the third column, divided by the number of lines in the ſecond column.

Obſervations on the barometer, made at	Lines of mercury	Said to correſpond with feet	The fall of one line of mercury correſ. with feet
I *Collioure.*	$3\frac{1}{3}$	195	58, 5
II The tower of *Maſſane*	31	2382	76, 8
III *Bugarac*	42	3636	86, 5
IV *Rupeyroux*	30	2181	72, 7
V *Rodez*	24	1647	68, 6
VI *Rodez*	20	1425	71, 25
VII *Courelande*	54	4812	89, 1
VIII *Coſte*	54	4890	92, 4
IX *Clermont*	3	200	66, 6

A ſight of this table will convince any one, that theſe obſervations are not to be depended on, for determining the height of the mountains in the ſouth of *France*: For, the differences are not ſmall, ſuch as might happen in making the experiments; but ſuch, as render the obſervations, uſeleſs for the abovementioned purpoſes. For example, the firſt and ſeventh differ almoſt $\frac{1}{3}$; and if 58, 5 feet were allow'd for the fall of one line of mercury in the ſeventh obſervation, inſtead of 944 feet, then the mountain of *Coſte* would be but 3085 feet, inſtead of 4890. Nay, upon examining the Memoirs, the Dr. finds, that in ſeveral obſervations, the number of toiſes ſaid to correſpond to a certain height of mercury, are only anſwerable to the height of the mountain above the level of the ſea, found by trigonometry, from which the height of the Royal Obſervatory above the ſea is ſubſtracted; tho' by the manner of expreſſion a curſory reader would imagine, that the number of toiſes nam'd, was always proportionable to the fall of the mercury, and think all the experiments and obſer-

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vations very accurately made, when they ſeem to agree ſo well in every reſpect.

Now after all, the Dr. does not queſtion, but that the height of the barometer might be, as it is ſet down in the Memoirs, and well enough obſerved. But it was wrong to compare the height of the mercury in the ſouth of *France*, with the height the mercury was at, in the barometer of the Royal Obſervatory at the ſame time: For, at that great diſtance and difference of Lat. the weather (and conſequently, the preſſure of the atmoſphere and height of the barometer at the ſame level) might vary very much.

Even when there is fair weather all over *France*, it does not follow, that the barometer ſhall ſtand at the ſame height. Let us ſuppoſe, for example, that a north wind blows; whereever the air is check'd by a chain of mountains that run eaſt and weſt, it will be accumulated over thoſe mountains, and conſequently, preſs more, as its columns are higher; which will make the mercury riſe higher than it would do with the ſame wind, if there were no mountains, or if they ran north and ſouth.

The way to have made the experiments with the barometer exactly, would have been to have obſerved the height of the mercury, at the top and at the bottom of the mountain, and that with a tube of a pretty large bore, with a proportionably large ciſtern for the ſtagnant mercury; becauſe, in a ſmall tube, the mercury will often adhere to the ſides, and riſe irregularly, as it will alſo in inclin'd barometers: Simple barometers are the beſt, and a magnifying glaſs may be made uſe of to obſerve ſmall riſes or falls, having two fine and well made indices to the tube.

Dr. *Halley* has, in a former Tranſaction, exhibited the falls of mercury in the barometer, correſponding with the heights to which the barometer muſt be carried to produce thoſe falls. He makes the firſt tenth part of an inch in the fall of mercury to correſpond to a height of 90 foot; the next tenth, to a height ſomewhat greater; and ſo in proportion, as the air diminiſhes in denſity, according as we riſe in height. He has built the proportion of the firſt tenth of the mercury's fall upon the compariſon of the different ſpecific gravities of air and mercury; and taking mercury to be $13\frac{1}{2}$ times heavier than water; and water in cold weather to be 800 times heavier than air; it follows that $13,5 \times 800$ will give 10800; which number, if taken in feet and divided by

120

120 (the number of the tenths of an inch in a foot) we ſhall have 90 feet correſponding to the tenth part of an inch, and 75 feet to a line or the twelfth part of an inch.

Now, as very few mountains in the world are three miles high; and generally ſpeaking, thoſe we look upon as high (excepting the *Andes* and ſome others in *America*) are not much above a mile; we may, for finding the height of mountains, take a fix'd number of feet in altitude to correſpond to every tenth or twelfth part of an inch in the fall of the mercury; becauſe 90 feet only are taken by Dr. *Halley* for the firſt tenth, and greater heights for other tenths, encreaſing with the fall of the mercury; Dr. *Deſaguliers* therefore would propoſe another for a round number, namely 96 feet for every tenth, and 80 feet for every twelfth of an inch, very near the number he found by calculation, as follows.

Fine mercury (ſuch as is made uſe of in barometers) is generally ſpeaking $13\frac{2}{3}$ times heavier than water; and he found ſome brought from the *Eaſt-Indies* to be 14 times heavier. He found air in ſummer to be near 900 times lighter than water; and 800 times lighter in winter; he therefore takes 850 at a medium. Now $850 \times 13\frac{2}{3} = 11606,6$, which divided by 120, gives 96, 7 feet, for one tenth of an inch of mercury, or 80, 5 feet for one twelfth of an inch. This number, taken invariable, will, in taking the height of ſeveral hills, agree pretty well with the numbers that come out, when Dr. *Halley*'s table is made uſe of; and with the experiment made by Mr. *Caſwell*, who, having taken the height of *Snowdon-hill* in *Caernarvonſhire* very accurately, and finding it to be 3720 feet above the level of the ſea, tried how much lower the mercury would ſtand in the barometer upon that hill, than at the level of the ſea, and he obſerv'd it to ſubſide 3, 9 inches. The Dr. is ſenſible, that it will be alledged, that the air will be denſer than he may imagine on the top of high hills; becauſe of the great cold, ſince they are generally cover'd with ſnow: But then we are to conſider, that when we are got above a mile higher than the level of the ſea, the incumbent atmoſphere has loſt almoſt a fifth part of its weight; and therefore, the air, at the top of the hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe cold, be more rarified than at the bottom of the hill.

Now, if we go back to the obſervations of the barometer, made by the gentlemen that drew the meridian in *France*, we ſhall find, that on the mountain of *Rodez*, in Lat. 44° 21' the

the barometer fell 24 lines below the level of that in the Observatory, and they allow'd only 274 toises and ½ to correspond to that fall: Whereas, according to Dr. *Halley*'s proportion of a tenth of an inch for 90 feet, they should have taken 300 toises; and tho' the hypothenuses A B and BC (Fig. 15.) were taken longer than the bare declivity of the mountain, (which would make the error less than the 79 toises above-mentioned) yet if Dr. *Desaguliers*'s proportion be made use of, *viz.* 80 feet for each line of mercury, it will make the mountain 320 toises, which being higher, will, therefore, shew the base to be yet shorter; and consequently, the error at that rate will be greater.

This error (and such like, if any more were made) will increase the measure of the 44th degree of Lat. on the earth; and by observing what was done in the next degree, we shall find, that that degree was taken too short. In the Lat. of 45° 38′ the mountain of *Coste* is made 815 toises high; whereas the 54 lines of the falling mercury in the barometer, said to answer to that height, will give but 705, 6 toises (which we will call 705, 5) even according to the Dr's computation of 80 feet to a line, which is the greatest allowance. If we suppose this mountain to rise in an angle of 26° 33′, as we did that of *Rodez*, the sides of the mountain, or hypothenuses A B and B C (Fig. 16.) will be each equal to 1577, 54 toises, and the whole base A C to 2822 toises. Now, when the height of this mountain is call'd 815 toises, the base A D or D C (by *Eucl.* 47. 1.) becomes only equal to F D or D G = 1350, 7; and its double or F G, the whole base, will be but 2701, 4 toises, less than the former by 120, 6 toises.

This error is so great (so much more than the difference of 31 toises for a degree) that tho' the Dr. supposed the lines found by trigonometry, which terminate at the top of the mountain, to be much longer than the hypothenuse A B, yet there will be error enough to make the 45th degree of Lat. appear much shorter than it really is, supposing (because of the length of the lines A B, or the great distance from which the mountain might be observ'd) that these errors were four times less than the Dr. has made them; yet at that rate, one must add near 20 toises to the 44th degree of Lat. and take away above 30 from the 45th degree, which will make the 44th of 57080 toises, and the 45th of only 57030; and this will give a difference of 50 toises: So that if an angle can be taken to two or three seconds, to which 32 or 48 toises, are said

ſaid to anſwer upon the ſurface of the earth, ſuch a difference might be viſible; and much more ſo, if other errors of the ſame kind ſhould happen to have been made the ſame way; or if thoſe errors were nearer his firſt ſuppoſition than this laſt. Nay, tho' the 45th degree of latitude may be 13 toiſes larger than the 44th, it might by this means appear to be conſiderably leſs.

Such a miſtake might be the occaſion of making the hypotheſis of the earth being an oblong ſpheroid; eſpecially, becauſe in this hypotheſis, the degrees differ moſt in length from each other about the 45th degree; and when once an hypotheſis is ſet on foot, we are too apt to draw in circumſtances to confirm it; tho' perhaps, when examined impartially, they may rather weaken than ſtrengthen our hypotheſis; otherwiſe the author of the hiſtory of the *Royal Academy* for the year 1713, would not have alledged, that M. *Caſſini* the elder had obſerved *Jupiter* to be oval, as a proof of young M. *Caſſini*'s hypotheſis; becauſe *Jupiter* is oval the other way, that is, an oblate ſpheroid, flatted at the poles, as the ſaid M. *Caſſini* the elder gave the proportion of the axis to the equatorial diameter to be as 15 to 16: And Mr. *Pound*, with a teleſcope of 123 foot *focus* and an excellent micrometer, has given thoſe proportions, as 11 to 12. If a proof is to be drawn from analogy, or what is obſerved in other planets, this muſt deſtroy M. *Caſſini*'s hypotheſis and confirm Sir *Iſaac Newton*'s.

The opinion of Dr. *Burnet*, quoted in the *Memoirs*, for the year 1713, is but a very weak argument, in favour of M. *Caſſini*'s hypotheſis, on account of the reaſon Dr. *Burnet* gave, to prove the earth higher at the poles than at the equator: For, he ſays, 'that the velocity of the parts of the earth, in its diurnal rotation, being greater at the equator than towards the poles, all the water muſt be driven towards the equatorial regions; from whence being repelled by the reſiſtance of the air, it muſt run off again towards the poles; and ſo the figure of the water was lengthened out into an oblong ſpheroid; and conſequently, the cruſt of the earth over it did put on the ſame figure, &c.'

But why the air ſhould reſiſt more towards the equator than the poles, Dr. *Burnet* did not give any reaſon; and if it had been ſo, the ſame force that drove the water towards the equator, muſt have kept it there. The Dr. in the latter part of his aſſertion, forgot what he had ſaid in the former: For, the water could not run off towards the poles, whilſt the earth continued

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its rotation with the ſame velocity; for, if he had conſidered, he would have found his argument in other words to run thus; 'becauſe bodies, that move in a circle, always endeavour to recede from the axis of their motion; therefore, the water, by 'that endeavour, comes nearer to the axis of its motion;' which is abſurd. But Dr. *Burnet* afterwards altered his opinion, as Dr. *Deſaguliers* was credibly informed.

Dr. *Deſaguliers* having thus given his reaſons for diſapproving of M. *Caſſini*'s opinion, concerning the figure of the earth; comes now to conſider Sir *Iſaac Newton*'s, who makes it higher at the equator, than at the poles; but before he enters upon it, he quotes a paragraph out of the *hiſtory of the Royal Academy* for the year 1713: 'Reaſonings, *ſays the Author*, drawn 'from the different lengths of a *pendulum* in different climates, 'or from the inequality of the centrifugal force, ariſing from 'the diurnal motion of the earth, are, perhaps, too nice to 'produce a certain conviction; nay, perhaps, we are not well 'enough aſſured of the principles; and the conſequences may 'ſometimes be different: And therefore, it is evident, that the 'beſt way in this enquiry, is only (as M. *Caſſini* does) to make 'uſe of unqueſtioned obſervations, which ſerve directly to decide the matter.'

That M. *Caſſini* has not made uſe of unqueſtioned obſervations; and that the meaſures he mentions are not able to decide the queſtion, appears from what has been already ſaid. We muſt, therefore, ſhew, whether the principles, from which Sir *Iſaac Newton* has deduced his figure of the earth, be fully proved or not; whether the concluſion drawn from them be plain and evident; and whether the experiments on pendulums that confirm the theory, be eaſy to be made and ſuch as may be depended on.

Tho' Sir *Iſaac Newton*, in his *Principia*, has not endeavoured to give the cauſe of gravity, or to determine, whether it be owing to an impulſe or not; yet he has ſhewn what its effects and laws are, from plain experiments made by others and himſelf. From the laws of gravity and from the obſervation of a comet (vide *Princip. lib.* 3. *Prop.* 12, 13 and 42) he has deduced the annual motion of the earth; and it muſt have a diurnal motion, if it have an annual one; otherwiſe it will not agree with the phænomena. The laws of the centrifugal force, or that force by which a body, whirl'd round in any circle, endeavours to recede from the center of its motion, have been demonſtrated by M. *Huygens*.

These are the principles from which Sir *Isaac Newton* draws his conclusion; and tho' some persons, that will not be at the pains to examine them, may deny them in the lump; yet no body has yet been able to shew any flaw in the demonstrations that relate to them.

The Dissection of an Ostrich; *by Mr.* John Ranby. Phil. Trans. N° 386. p. 223.

MR. *Ranby*, having separated the muscles of the *abdomen*, which in this subject were only two oblique pair, observed between their tendons, which were very strong, and the *peritonæum*, which was exceeding thin, a thick layer of serous fat, whose office, considering the smallness of the *epiploon* and the few adipose vesicles of the mesentery, with the thinness of the *peritonæum*, might, probably, be to supply the place both of *epiploon* and mesentery in other animals, and lubricate the intestines.

In this subject there were two distinct ventricles, contrary to the observation of the *Royal Academy* at *Paris*; the first, and in its natural situation, the lower, which the Members of the said *Academy*, call the craw, and suppose to be only a dilatation of the *oesophagus*, was considerably larger than the second and uppermost muscular one; besides that it had strong muscular fibres, both circular and longitudinal: The *duodenum* comes immediately out of the second ventricle.

Both ventricles were distended beyond their usual form and filled up with so large a quantity of food of different kinds, as stones, bones, sticks, grain and other food, that it was almost impossible for them to perform their office of digestion, which, very likely, was one of the chief causes of the animal's sickness and death; and indeed, the contents of both seemed to have undergone but very little or no alteration. The *epiploon* partly covered the first ventricle; but was no ways proportionable to the size of the animal.

The spleen was fastened by a membrane to the right side of the second ventricle; and was very small, considering the size of the animal.

The glands of the mesentery were hardly visible; but the veins and arteries were very discernible.

The *cœcum*'s in this subject were near three foot in length, and one inch eight lines in diameter; they were fastened to the *ileum* and not to the *colon*, as the Gentlemen of the *Royal Academy* assert.

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To their deſcription of the kidneys Mr. *Ranby* has nothing to add, only that the two *ureters* lay upon their ſurface, as they do in other birds; and by their different branches, coming from all the parts of the kidney, of which the ſuperior was very diſcernible, entered the kidney about its middle, and formed there a very large *pelvis*.

The liver was in the ſame cavity with the heart, one half of which it nearly covered; it had no gall-bladder, and but one *ductus bilarius* inſerted into the *duodenum*, about two inches below the *pylorus*, which ſeemed to have an immediate communication with the *vena portæ*; becauſe by blowing into it, this latter was alſo diſtended. The heart and liver were ſeparated from the inteſtines by a membranous diaphragm.

Both the heart and liver were ſuſpended by one common *mediaſtinum*, by the help of its ſeveral membranes, and 8 ſtrong muſcles on each ſide, ariſing from the upper part of the ribs, going from thence over the lungs, and ending in a very ſtrong tendinous membrane, which is inſerted into the *ſpina dorſi*.

The liquor, contained in the *pericardium*, was ſmall in quantity and perfectly tranſparent.

The lungs lay under the diaphragm and its muſcles in a deep cavity, formed by the five true ribs: They were pretty thick about the middle, and exceeding thin and ſharp towards the extremities.

In viewing the external eye, it ſomewhat reſembled the human, only that it was leſs convex, with a free and moveable upper eye-lid, with eye-laſhes, as moſt terreſtrial animals have, beſides a *tunica nictitans*, as in other birds. Beſides the ſeveral muſcles of the eye, as they are in brutes, it had two more, one ariſing from the fore-part of the *ſclerotica*, which ſoon formed a ſmall tendon, obliquely ſurrounding the optic nerve, and then joined to another muſcle, which ariſes oppoſite to the former, from which the tendon continues its way and is inſerted in the *tunica nictitans*: The aqueous humour was found in greater quantity than uſual: The cryſtalline humour was of an uniform ſubſtance; but leſs convex on the inſide than on the outſide: The vitreous humour was ſmall in quantity, conſidering the largeneſs of the eye; the *choroides* was entirely black, without that variety of colours at its bottom, which is common to moſt brutes. The fore-part of the *ſclerotica*, where it is annexed to the *cornea*, was bony, conſiſting of 15 bony ſcales, joined to each other; ſo as to form one circular bone round the *cornea*.

Fig. 1. Plate XIII. (after removing the *sternum*) represents the upper part of the *thorax*, with the heart and liver and neighbouring parts, in their natural situation; A A the membranous diaphragm, in which were observed several distinct cavities; *a a a* the ligament that suspends the diaphragm; *b b* the ribs; B the heart; C C the two lobes of the liver, immediately above the heart; *c c* the brachial artery; *d* the vein; *e e* the *vena cava*; *f* a gland on the brachial artery; *g g* part of the *aspera arteria*; *h h* part of the *oesophagus*; *i i* two muscles arising from the *sternum*, and inserted into the *aspera arteria*.

Fig. 2. Represents the inferior part of the *thorax* (the heart and liver being removed) A A A the lower part of the diaphragm, immediately covering the lungs; B B, &c. 8 strong fleshy muscles, arising from the ribs, and inserted into the diaphragm, forming a cavity for the heart and liver; *c c*, &c. the ribs; D the descending trunk of the *aorta*; E E the left lobe of the lungs, freed from the diaphragm; F part of the *aspera arteria*.

Fig. 3. Represents part of the globe of the eye, *a* the *cornea*; *b b* the *ligamentum ciliare*; *c c c* the fore-part of the *sclerotica*, composed of 15 bones.

Fig. 4. Represents the back part of the globe; *a a a* the back part of the *sclerotica*; *b b b*, &c. the 7 muscles; *c c c* the 8th and 9th, whose tendon *d d* goes round the optic nerve *f*, and is inserted into the *tunica nictitans*; *e e* the *membrana nictitans*.

Fig. 5. Represents the kidneys with their vessels; A A the kidneys; B B the descending *aorta*; C C the *cava*; D D the emulgent arteries; E E the emulgent vein with its ramifications; F F the *ureters*; G the union of the superior and inferior *ureter*.

The mean Motion of Mercury *and his* Nodes, *determined by a transit of that Planet over the Sun's Disk*, October 29. 1723; *by Dr.* Halley. Phil. Trans. N° 386. p. 228.

THE transit of the planet *Mercury*, over the sun's disk, being one of the most curious and uncommon appearances that the heavens afford, our astronomers, both at home and abroad, made due preparation to observe, with the utmost exactness, that which happened on the 29. of *October* 1723; and which Dr. *Halley* had predicted in the year 1691 (vide *Phil. Trans.* N° 193) would in part be visible in *England*; and the sky, proving more than ordinary favourable at that time, we were enabled to observe the ingress on the sun's limb with the greatest accuracy. Accord-

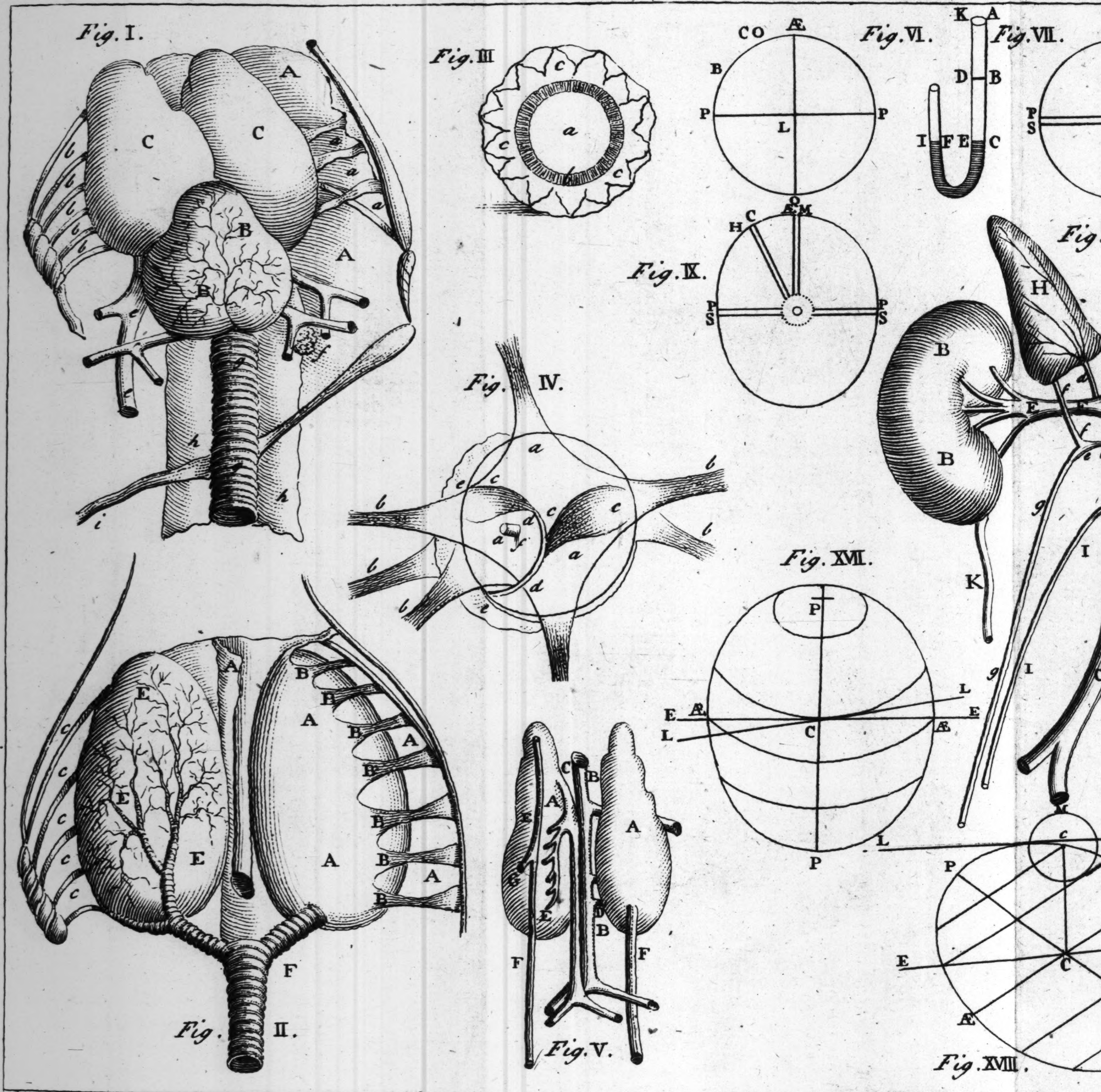
Fig. I.
Fig. II.
Fig. III
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Fig. VII.
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Fig. XVI.
Fig. XVIII.

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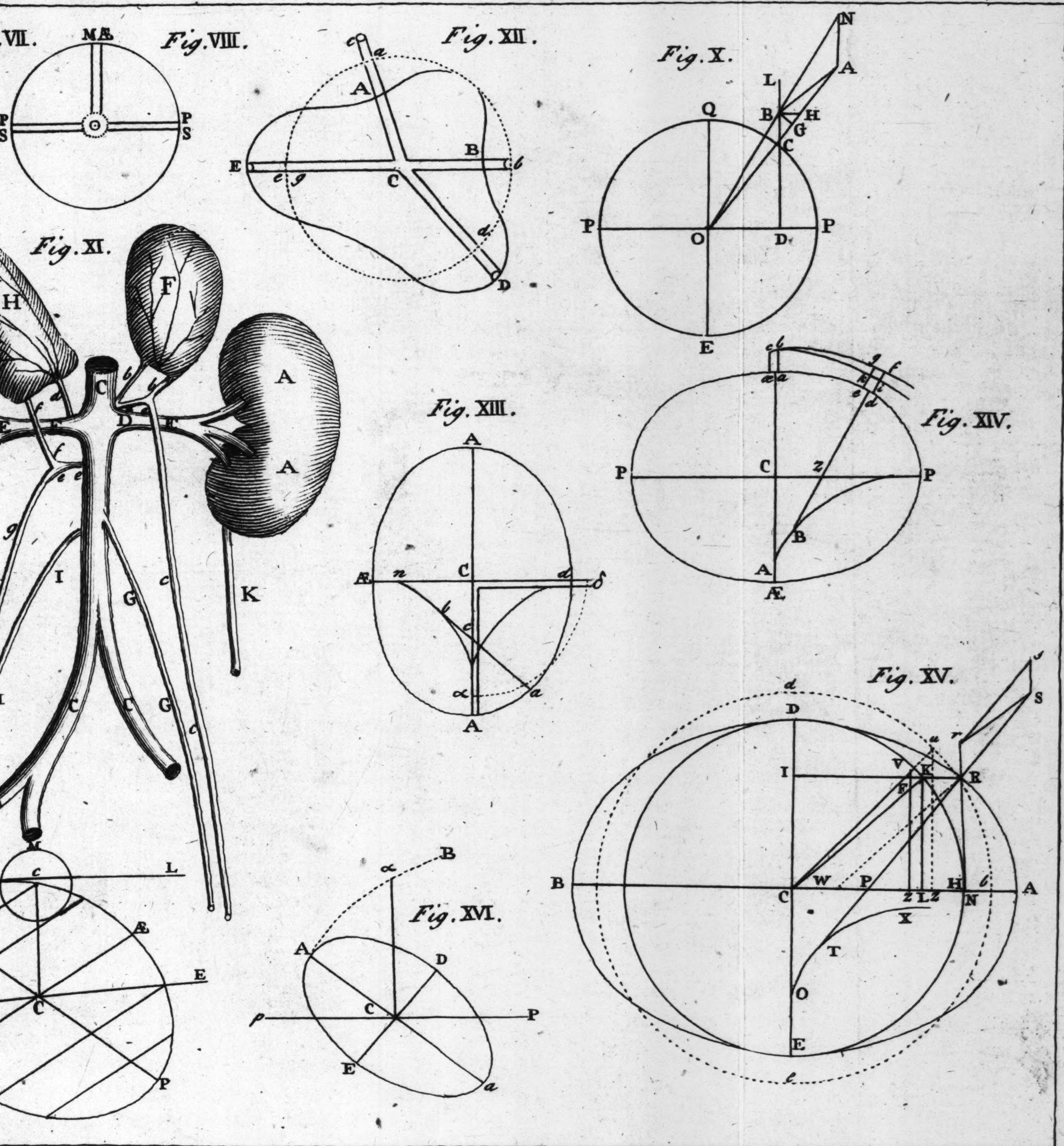

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Accordingly, the ſame day, *viz.* *October* 29. 1723. O. S. in the *Royal Obſervatory* at *Greenwich*, the Dr. firſt perceived with his 24 foot teleſcope, the planet making a ſmall notch in the ſun's limb at 2^h 41′ 23″ apparent time; and at 2^h 42′ 26″ he was entirely entered, making an interior contact, the light of the ſun's limb juſt beginning to appear behind his dark body; which, notwithſtanding the ſlowneſs of the motion, was, in a manner, inſtantaneous: Then applying the micrometer to the ſaid 24 foot tube, he opened it in ſuch a manner, as to take in 16′ 15″, equal to the ſun's ſemidiameter at that time; and cauſing the northern edge of the ſun to move exactly along one of the pointers, he waited till the center of *Mercury* came to move along the other, as he found it to do at 3^h 1′ 16″ apparent time. But refraction contracting this difference of declination about 5″ (the ſun being then but about 11° high) he concluded, that the centers of the ſun and *Mercury*, were truly in the ſame parallel of declination at 3^h 3′ apparent time nearly.

At *Wanſted* in *Eſſex*, Mr. *Bradley* obſerved with the *Huygenian* teleſcope, upwards of 120 foot long, the total immerſion or interior contact of the limbs, at 2^h 26′ 45″ equat. time, that is 2^h 42′ 38″ apparent time, twelve ſeconds later than Dr. *Halley* found it at *Greenwich*; moſt of this difference was owing to the difference of meridians: And Mr. *Bradley*, applying the micrometer to that vaſt radius, meaſured the diameter of the planet 10″ 45‴. At 2^h 48′ 57″ he found the difference of declination between the ſouthern limbs of the ſun and planet by the micrometer, in a 15 foot teleſcope, to be 15′ 19″. Wherefore, allowing the obſerved ſemidiameter of the planet and the refraction, the ſaid difference was neareſt 15′ 30″; and conſequently, *Mercury* 45″ more ſoutherly than the ſun's center, in reſpect of declination.

In *Fleet-ſtreet* Mr. *George Graham* obſerved the firſt impreſſion on the ſun's limb at 2^h 41′ 9″ apparent time; and at 2^h 42′ 19″ *Mercury* was entirely within the diſk. At 3^h 6′ 41″ he meaſured with a micrometer, in a 12 foot teleſcope, the diſtance of his center from the neareſt limb of the ſun 2′ 13″. And again at 3^h 25′ 24″ their diſtance was found 3′ 57″. At 3^h 34′ 43″ he meaſured the difference of declination from the northern limb of the ſun 14′ 57″, which, corrected by refraction, becomes 15′ 4″, that is, 1′ 11″ more northerly than the ſun's center.

In the *Obſervatory* at *Paris*, S. *Maraldi* obſerved the firſt appearance of *Mercury* on the ſun's limb, at 2^h 50′ 13″ apparent

rent time, and the interior contact at $2^h\ 51'\ 48''$. And M. *de Lisle*, observing a-part, concluded the same at $2^h\ 51'\ 37''$; but suspects it might have been some few seconds later: The last mentioned Gentleman's observed latitudes were, as follows.

	h.	′	″		′	″
At	2	56	20	the N. Lat. of *Mercury* was	3	36
	3	0	40		3	42
	3	10	20		3	46
	3	16	12		3	55

At *Bononia* in *Italy*, S. *Manfredi* observed *Mercury* indenting the sun's limb at $3^h\ 26'\ 22''$, and that he was gotten entirely within it, at $3^h\ 27'\ 45''$. And these are the observations (most to be depended upon) Dr. *Halley* received from abroad.

In order to deduce from this phænomenon, so accurately observed, what may contribute to the perfecting of the theory of *Mercury*'s motion, which (as appears by the near agreement of the Dr's numbers with this and several other observations of him) seems to need but very little correction. The Dr. carefully computed from his tables the motion of the planet in five hours, and found his apparent motion on the sun to be in longitude $29'\ 21''$ retrograde; and that his latitude increased northerly $4'\ 17''$ and $\frac{1}{2}$ in the same time: Whence the horary motion in longitude $5'\ 52''$ and in latitude $51''$ and $\frac{1}{2}$; and thence the angle of the visible way, with the ecliptic $8^\circ\ 19'$; and the horary motion in that way $5'\ 56''$. Again, the angle of the ecliptic with the meridian being in this place $73^\circ\ 24'$, the visible way of *Mercury* made an angle of $65^\circ\ 5'$ with the meridian, passing thro' the center of the sun, whence the horary change of declination becomes $2'\ 30''$ exactly.

These *data* the Dr. chooses rather to take from the theory than from immediate observation; because there is always an unavoidable, tho' small uncertainty, in what we observe; yet greater than there can be in the computation for so small a space of time; especially now the theory is, as has been said, so very near the truth.

This premised, let us now enquire into the true time of the central ingress, and the latitude of the planet at that time. And first by Dr. *Halley*'s own account, *Mercury* was gotten into the parallel of the sun's centre $21'$ and $\frac{1}{2}$ after the central ingress, in which time he ascended to the northward $54''$; and so much, therefore,

therefore, was he more ſoutherly than the ſun's center at his ingreſs. Mr. *Bradley*, 7′ and ½ after the ſaid ingreſs, in which the planet aſcended 19″, found his declination 45″ ſouth; and, therefore at the ingreſs, his declination was 1′ 4″ ſouth. And by Mr. *Graham*'s obſervation, *Mercury* was more northerly than the ſun's center 1′ 11″, 53′ 20″ after the central ingreſs; but in that time, *Mercury* aſcended 2′ 13″; wherefore, according to him, at the ingreſs the planet had 1′ 2″ S. declination. We ſhall not, therefore, miſtake above a ſemidiameter of *Mercury*, if we aſſume his declination at that time to have been preciſely one minute.

Now the ſun's ſemidiameter being then 16′ 15″, 1′ is the ſine of 3° 32′ in the arch of the ſun's limb; and conſequently, the point of this ingreſs was 13° 4′ more northerly than the ecliptic: Whence the latitude of *Mercury* was then 3′ 40″. N. and difference of longitude 15′ 50″, by how much he, at that time, followed the ſun's center.

If therefore, to the arch of 13° 4′, we add the double of 8° 19′, or of the angle, which the viſible way made with the ecliptic, we ſhall have 29° 42′ for the point on the ſun's weſtern limb, at which the planet made his exit, likewiſe to the N. of the ecliptic: Hence the chord, deſcribed in the whole tranſit, was of 137° 14′, and the chord itſelf 30′ 16″; and the neareſt diſtance to the ſun's center 5′ 56″. Now the horary motion in this chord being 5′ 56″, the whole duration of this mercurial eclipſe becomes 5^h 6′ in reſpect to the center of the planet; and therefore, the neareſt approach of their centers was at 5h. 14′ 30″ at *Greenwich*; and the exit at 7h. 47′ and ½, both viſible in our *American* plantations, had there been any curious perſons there, qualified to obſerve them.

It likewiſe follows, by the obſerved diameter of *Mercury*, *viz.* 10″ 45‴, that he was very little leſs than 2′ of time in paſſing the limb; and by the given neareſt diſtance to the ſun's center, it is concluded, that he was in conjunction, in point of longitude at 5h. 23′ 15″, having then 6′ N. Lat. preciſely: Nor can it be doubted, but that all this would have been found exceeding near the truth, had not the too early ſetting of the ſun deprived all *Europe* of the deſirable ſight.

There being a very remarkable period of the motion of *Mercury* in 46 years, in which time, he makes 191 revolutions about the ſun; this tranſit of ours is found to have been preceeded by two others at that interval. The firſt at *Paris* when *Gaſſendus* on the 28. of *October* 1631, O. S. was the firſt that ever

ever obſerved this appearance of *Mercury*, within the ſun's diſk, and found him to paſs off at 10h. 28′ *mane :* The ſecond was *October* 28, 1677, when Dr. *Halley* himſelf had the good fortune to obſerve both the ingreſs and egreſs of the planet in the iſland of St. *Helena* ; the mean time, when he was neareſt the ſun's center, being there 3′ 50″ paſt noon ; and the viſible duration of the tranſit of the center of the planet 5h. 14′ 20″, which was ſome ſmall matter contracted by parallax, and moſt likely might have been 5h. 15′ without it. Now in 5h. 15′ *Mercury* deſcribed the chord of 146° 52′ in the ſun's limb, being 31′ 9″ ; and conſequently, the neareſt diſtance to the center was 4′ 38″, or the ſine of 16° 34′, the ſun's ſemidiameter being radius ; that is, 1′ 18″ leſs than it was fouud in 1723. Hence alſo it follows, that the true conjunction in longitude was 7′ of time later than the neareſt approach of the centers, *viz.* at 10′ 50″ at St. *Helena* or at 35′ paſt noon at *Greenwich* ; and that the N. Lat. of the planet at that time was 4′ 41″.

Suppoſing, therefore, the neareſt diſtance of the centers in the tranſit of 1631 to have been 3′ 20″, that is, 1′ 18″ leſs than in 1677, we ſhall find, that *Mercury* then deſcribed a chord of 156° 20′, traverſing the diſk of the ſun in 5h. 21′ 30″ ; ſo that ſuppoſing his exit at 10h. 28′ at *Paris*, that is, 10h. 18′ 40″ at *Greenwich*, he entered on the ſun at 4h. 57′ 10″ in the morning ; and was neareſt his center at 7h. 38′ apparent time ; but in the ſame longitude with him at 7h. 43′, or *October* 27, 19h. 43′ apparent time. having then 3′ 22″ N. Lat.

Dr. *Halley* in *Phil. Tranſ.* N° 193. for the month of *March* 1690-91 ; that is, upwards of 30 years before, predicted, by help of the two former, this laſt tranſit, with a ſurpriſing exactneſs, even beyond his expectation, making the time of the middle or neareſt approach of the centers of the ſun and *Mercury*, *October* 29, 1723, 5h. 19′ apparent time, which was found by obſervation at 5h. 14′ and ½, only 4′ and ½ ſooner ; and in latitude, *Mercury* was but 6″ more ſoutherly than the Dr. had then computed it ; the error in longitude being little more than two diameters of this exceeding ſmall planet, and in latitude but a ſingle ſemidiameter thereof. So that, for the future, aſtronomers may depend upon Dr. *Halley*'s table of theſe tranſits in *Phil. Tranſ.* N° 193. to a few minutes of time, and not wait with the uncertainty of hours, nay days, as has lately been done.

But, in order to obtain a yet farther degree of exactneſs by help of this obſervation, it may be moſt expedient to compare with

with it the ingreſs the Dr. obſerved at St. *Helena*; becauſe in that, as well as in this, the latitudes of the planet being very ſmall, a little error in them will not ſo much affect the longitudes. Suppoſing, therefore, that *Anno* 1677, *October* 27, 21h. 26′ 15″ at St. *Helena*, or 21h. 50′ 15″ apparent time at *Greenwich*, the center of *Mercury* entered on the ſun; and that at that time, he was 8° and $\frac{1}{4}$ on the ſun's limb, to the north of the ecliptic (according to what is above concluded) it follows, that he had then 2′ 20″ N. Lat. and 16′ 5″ greater longitude than the ſun's center; as in the preſent tranſit, *October* 29, 2h. 41′ 30″ apparent time at *Greenwich*, he had 3′ 40″ N. Lat. and 15′ 50″ more longitude.

Now, the apparent geocentric differences of longitude are to the real heliocentric differences, as the planet's true diſtance from the ſun, to his diſtance from the earth; that is, in both caſes, as 313 to 676: Wherefore, in 1677, *Mercury* wanted 34′ 45″ of the conjunction with the ſun; and in 1723 but 34′ 13″, at the times of his apparent ingreſs on the diſk. And equating the times, the Dr. finds, that the ſun *Anno* 1677 *October* 27, 21h. 34′ 20″ equated time was in 15° 36′ 55″ of *Scorpio*; and conſequently, *Mercury*'s heliocentric place in 15° 2′ 10″ of *Taurus*: And *Anno* 1723, *October* 29, 2h. 25′ 30″ equat. time, the ſun was in 16° 39′ 43″ of *Scorpio*; and therefore, *Mercury* at that time in 16° 5′ 30″ of *Taurus*.

Mercury, therefore, in 46 years, with 11 intercalations, and beſides 1d. 4h. 51′ 10″, has performed 191 revolutions to the equinoctial points, and over and above 1° 3′ 20″. But by the *Scholium* to *Prop.* 14. *Lib.* 3. *Nat. Phil. Princip. Math.* the motion of the *Aphelion* of *Mercury*, from the equinox in that time, is 40′ 18″: So that there remains 23′ 2″ of true anamoly to be reduced to the mean. Now the mean anamoly of *Mercury*, in both caſes, being 5 ſigns 12° 23′ 2″ of true anamoly, gives 15′ 24″ mean anamoly; which added to 40′ 18″ becomes 55′ 42″, for the mean motion above ſo many revolutions; and this is to be increaſed by 8″ to reduce it to the plane of *Mercury*'s orbit, in all 55′ 50″.

Hence, doubling the interval, in 92 *Julian* years 1d. 9h. 42′ 20″, the mean motion of *Mercury* from the equinox is 1° 51′ 40″, from which taking 5° 44′ 50″, the motion in 1d. 9h. 42′ 20″, we have his motion in 92 *Julian* years 11^s 26° 6′ 50″; and in 100 years 2^s 14° 2′ 13″, which is but 20″ more than the Dr. had made it ſome years ago, in his aſtronomical tables,

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ſhortly to be publiſhed; and differs but one hour's motion therefrom in 3000 years.

The abovementioned proportion of the diſtances, *viz.* 313 to 676 is alſo between the latitudes, ſeen from the earth and the inclinations or heliocentric latitudes of the planet: So that 2′ 20″ at the ingreſs of 1677 gives 5′ 2″; and 3′ 40″ in 1723 becomes 7′ 55″ for the latitudes at the ſun. And the inclination of the orbit of *Mercury* to the plane of the ecliptic (determined by accurate obſervations near his northern limit) being 6° 59′ 20″, we compute the diſtance of the planet from his node, in the former 41′ 7″, and in the latter 1° 4′ 37″; which deducted from his heliocentric places reſpectively, leave the place of the aſcending node in 1677 14° 21′ 3″ of *Taurus*; and in 1723 15° 0′ 53″ of *Taurus:* So that in 46 years the node is found 39′ 50″ forwarder in the ecliptic; which is but 1′ 30″ more than the preceſſion of the equinox in the ſame time; we may, therefore, ſafely aſſume the plane of *Mercury*'s orbit to be immoveable in the ſphere of fixed ſtars, and its aſcending node to be 15° 41′ from the firſt ſtar of *Aries:* Nor can ſo very ſlow a motion (ſuppoſing any ſuch) be fully defined; but by the utmoſt care and diligence of future aſtronomers, after the obſervation of many ages.

As to the reſt of the theory of this planet's motion, Dr. *Halley* makes his mean diſtance from the ſun, 38710 ſuch parts, as the mean diſtance of the ſun and earth is 100000; and his greateſt equation 23° 42′ 37″. The *epocha* of his mean motion in the beginning of the year 1723, O. S. from the equinoctial point, he makes 19° 9′ 31″ of *Sagittarius*; and that of his *aphelion* to the ſame time 13° 3′ 34″ of *Sagittarius*; the *aphelion* moving, according to the order of the ſigns, 7 minutes in 8 years. And theſe numbers, the Dr. preſumes, may repreſent the motion of *Mercury*, with an exactneſs equal to that of any of the other planets; perhaps, as near as the ſun's place by any tables, or thoſe of the fixed ſtars by any catalogue yet extant.

It were to be wiſhed, that ſome good obſervation, like this, had been made of the like tranſit of *Mercury* at his other node in *April*; where he was ſeen it is true, *April* 23, 1661, but ſo imperfectly, that neither ingreſs or egreſs was any where obſerved; and tho' it be certain, that he traverſed the ſun on *April* 26, 1674; and again *April* 24, 1707; yet we were ſo unfortunate, that the conjunction in both happened ſo near midnight,

night, that he escaped unobserved by all the astronomers in *Europe*, excepting only M. *Roemer* at *Copenhagen*, whose observation is, as follows. 'This day, *viz. May* 6, 1737 at 4h. '19′ in the morning, *Mercury* was observed just going off the 'sun's limb; being $\frac{1}{4}$ of the sun's diameter above the lowest 'limb, and to the left in an inverting telescope; by reason of 'the too short *mora* we could not determine these things with 'greater accuracy.' It was great pity, that he did not at least estimate, how many diameters of his body he was distant from the sun's limb, or what part of the diameter, if so near. But Dr. *Halley*, having examined this observation, finds, that the sun at that time, was but just risen, or rather rising, and soon after entered into a cloud; so that the sun's limb could not be distinctly seen, it always undulating and sparkling much, when so near the horizon; in which circumstance, a just observation could hardly be made.

Let us now see, how the Dr's numbers, corrected as above, will represent this observation. *Anno* 1707. *April* 24. 16h. 19′ at *Copenhagen* is 15h. 28′ at *Greenwich*; but 15h. 24. 20″ equated time. To this time the Dr. finds the sun's true place 14° 50′ 1″ of *Taurus*, and his distance from the earth 101005. The correct *epocha* of *Mercury*'s mean motion for the year 1707 is 3^s 13° 18′ 45″, to which adding for the rest of the time 3^s 19° 9′ 28″, we shall have his mean motion at the time of the observation 2° 28′ 13″ of *Scorpio*; and taking his *aphelion* in 12° 49′ 49″ of *Sagittarius* therefrom, we have his mean anamoly 10^s 19° 38′ 24″, and thereby the equation to be added 12° 39′ 41″; and thence the place of *Mercury* in his orbit 15° 7′ 54″ of *Scorpio*: But the correct place of the descending node is 14° 46′ 25″ of *Scorpio*; and therefore, *Mercury* being 21′ 29″ past the node, had 2′ 36″ S. Lat. at the sun; and his place reduced to the ecliptic was 15° 7′ 45″ of *Scorpio*, that is, 17′ 44″ past the conjunction of the sun, which diminished in the proportion of 5567 to 4533, or of the distance of the planet from the earth to his distance from the sun, becomes 14′ 27″; and by so much was he past the conjunction, as viewed from the earth. Again, by the same proportion, his geocentric latitude at that time was 2′ 7″ S. and therefore, his apparent distance from the sun's center, was 14′ 37″, that is, but 1′ 18″ from his western limb: So that he might well be said, *jamjam exiturus*.

But that *Mercury* should at that time be so far northerly, as M. *Roemer*'s words import, was absolutely impossible; and Dr. *Halley* is apt to think, that so acute an astronomer, as

M. *Roemer* was, could not himself be the observer; but some person less acquainted with these matters; which the words *spectabatur Mercurius*, instead of *Mercurium vidi*, seem to import. If he then had had N. Lat. he must needs have been seen in the sun in *April* 1720, which we are assured he was not.

Lastly, the Dr. advertises, that on the last day of *October* 1736, *Mercury* will again traverse the northern part of the sun's disk, both ingress and egress being visible to all *Europe*.

A Continuation of the Account of the Figure of the Earth; *by Dr.* Desaguliers. Phil. Transf. N° 387. p. 239.

HOW the figure of the earth is deduced from the laws of gravity and centrifugal force, is very well shewn by Dr. *John Keill* in a book he published in 1698, against Dr. *Burnet*'s theory of the earth: It is true, he has made a mistake in that book, as to the measure of the degrees of an ellipsis; but all relating to the oblate spheroidical figure of the earth is right; and the small difference of taking 15 *Paris* feet for the space a body falls thro' in a second, instead of 15 feet 1 inch and 2 lines; and a number of feet, a little less than true, for the diameter of the earth (which was not so well known at that time) will noways invalidate his demonstration: His words are, as follows.

'To prove the earth higher at the equator than at the poles, 'I will first suppose, that at the beginning of the world the 'earth was fluid and spherical; but afterwards God Almighty, 'having given it a motion round its own axis, all bodies upon 'the earth would describe either the equator, or circles parallel 'to the equator; and consequently, they would all endeavour 'to recede from the center of their motion.

'It is here to be observed, that if a body doth freely revolve 'in a circle about a center, as the planets do about the sun, that 'its centripetal force (or that force by which it is drawn towards 'the center) is always equal to the force, by which it endea-'vours to recede from the center: For, the force which detains 'a body in its orbit must be equal to the force by which it en-'deavours to recede from its orbit and fly off in the tangent. 'This may be made clear by a body turned round a center by 'the help of a thread, which retains the body in its orbit; the 'thread being stretched by the motion of the body, will endea-

'vour to contract itself equally towards both ends, by which it 'will pull the center as much towards the body, as it does the 'body towards the center.

'Now this centrifugal force is always proportional to the periphery, which each body describes in its diurnal motion by '*Huygens*'s first *Theor. de vi centrifugâ*: So that under the 'equator, which is the greatest circle, the centrifugal force 'would be greatest, and still become less, as we approach the 'pole, where it quite vanishes; there being no diurnal rotation 'there. And without doubt, all bodies having this centrifugal 'force, by which they endeavour to recede from the center of 'their motion, would fly off from the earth, if they were not 'kept in their orbit by their gravity, or that force by which 'they are pressed towards the center of the earth, which is 'much stronger upon our earth than the centrifugal force; and 'because the gravity upon the surface of the earth is always the 'same; but the centrifugal force alters and becomes less, the 'nearer we come to the poles; it is plain, that the gravity under 'the equator, having a greater force to oppose it, than that 'which is near the poles, will not act so strongly in the one 'place, as in the other; and consequently, bodies will not be 'so heavy under the equator, as at the poles. If the circle 'ÆPQP (Fig. 6. Plate XIII.) represent the earth, ÆQ the 'equator, and PP the poles; if C be a body in the equator; 'it is evident, that it will be pulled by two contrary forces; 'namely, that of its gravity, which pulls it towards the center, and that of its centrifugal force, which pulls it from it. 'Now, if both these forces were equal, it is evident, it would 'go neither of these ways: But if one were stronger than the 'other, it would move where the stongest force pulls it; but 'only with a velocity, which is proportional to the difference 'of these two forces; and therefore it would not descend so 'fast, as if there were no centrifugal force, pulling against it; 'that is, a body in the equator presses less towards the center 'than at the pole; where there is no centrifugal force to lessen 'its gravity: Bodies, therefore, of the same density, are not so 'heavy in one place as in the other.

'Now in a spherical fluid, all whose parts gravitate towards 'the center, I think it is evident from the principles of hydro-'statics and fluidity, that all those bodies, which are equally 'distant from the center, must be equally pressed with the 'weight of the incumbent fluid; and if one part come to be 'more pressed than another; that which is most pressed will

'thrust

'thrust out of its place that which is least, till all the parts
'come to an *æquilibrium* one with another; and this is known
'by a common and easy experiment: If you take a recurved
'tube, (represented Fig. 7.) and fill it with water or any
'other fluid, it will rise equally in both legs of the tube; so
'that the surfaces C E and F I are equally pressed by the in-
'cumbent columns B C E D and G F I H; but if one of the
'legs of this tube should be filled with oil, or some other
'lighter fluid, and the other with water; the lighter fluid
'will rise higher than the other: For, otherwise these sur-
'faces, which are equally distant from the center, would not
'be equally pressed.

'Just so, if P Æ M P S (Fig. 8.) represent a fluid sphere,
'which we may imagine composed of a great many commu-
'nicating canals or tubes, the fluid of every one of which
'presses upon the center: Now, if the fluid, in every one
'of these tubes, was of equal weight or gravity, it is plain,
'that by that means they would likewise be of an equal
'height from the center: For, by that means only, would the
'center be equally pressed by the weight of all the tubes;
'but if the fluid in the canal Æ O M were lighter than the
'fluid in the canal P O S, it is plain, that in this case, the
'fluid P O S pressing more upon the center, than the fluid in
'the canal Æ O M, the surface of the fluid Æ O M will rise
'to a greater height or distance from the center: So that by
'its greater height, which compensates its lesser gravitation,
'it will press equally upon the center with the fluid in the
'canal P O S.

'After the same manner, if the fluid in the canal G O H
'(Fig. 9.) were heavier than the fluid in the canal Æ O M;
'but lighter than that in P O S, then would the canal G O H
'be shorter than Æ O M, but longer than P O S, and the fi-
'gure, compos'd of all these tubes, would be in the form of a
'spheroid, generated by the circumrotation of a semi-ellipsis
'round its axis: But as has been already shewn, if Æ O M
'represent the semidiameter of the equator, all bodies in it
'are lighter than in P O S, the axis of the equator, taking
'the diameter and axis here, not as pure mathematical lines,
'but as small canals or tubes; and just so those bodies which
'are in the tube G O H, I have prov'd to be lighter than
'those in P O S, but heavier than the bodies in Æ O M; the
'centrifugal force in G H being less than that in Æ M, and
'there

'there is no centrifugal force in the poles PS. It is 'plain, therefore, that the tube ÆOM will be longer than 'GOH, and GOH longer than POS, that is, the diame- 'ter of the equator will be longer than the axis of the earth; 'and consequently, the figure of the earth will be in the form 'of a broad spheroid, generated by the rotation of a semi- 'ellipsis round its lesser axis. This, I hope, will be sufficient 'to convince the Theorist of the falseness of his own asser- 'tion; since it is plain demonstration, that an earth, form'd 'from a chaos, must have a very different figure from what 'he supposes it had.

'But I will now proceed farther, and enquire how much 'the gravity is diminish'd at the equator, or any other paral- 'lel by the centrifugal force, which all bodies acquire by 'being turn'd round the earth's axis, that from thence we 'may endeavour to determine, what proportion the diameter 'of the earth's equator hath to its axis, to calculate which, 'I will first suppose, that the mean semi-diameter of the 'earth is 196,115,800 *Paris* feet, according to the late ob- 'servations of the *French* Mathematicians; and since the 'earth turns round its axis in the space of 23 hours 56'; for, 'in that time the same meridian returns to the same immove- 'able point of the heavens again (but the sun, in the mean 'time, seeming to be mov'd a degree, according to the order 'of the signs, is the cause, why there are four minutes more 'requir'd, before the meridian can overtake him) thence it 'follows, that a body under the equator moves thro' 1426,88 'feet, in the space of one second of time.

'Now, according to the theorem given us by Sir *Isaac* '*Newton* in his *Philos. Natural. Mathem. Schol. Prop.* 4. '*lib.* 1. the centrifugal force of any body, has the same pro- 'portion to the force of gravity, that the square of the arch, 'which a body describes in a given time, divided by its dia- 'meter, has to the space, thro' which a heavy body moves, 'in falling from a place in which it was at rest in the same 'time; and supposing a heavy body falls 15 foot in a second 'of time, by calculation, it will thence follow, that the force 'of gravity has the same proportion to the centrifugal force 'at the equator, that 289 has to unity; and therefore, by this 'centrifugal force, which arises from the diurnal rotation of 'the earth round its axis, any body, placed in the equator, 'loses $\frac{1}{289}$ part of its gravity, which it would have, were the 'earth at rest; or which is the same thing, a heavy body,

'placed

'placed at either of the poles (where there is no diurnal 'rotation, and consequently, no centrifugal force) which 'weighs 289 pounds, if it were brought to the equator, would 'weigh only 288 pounds.

'Having thus determined the proportion of the centrifugal 'force at the equator, to the force of gravity, it will be easy 'from thence to shew their proportions in any parallel; for, 'it is compounded of the proportion of 1 to 289, and of the 'co-sine of Lat. to the radius; for, if two bodies describe 'different peripheries in the same time, their centrifugal 'forces are proportional to their peripheries, or to the semi-'diameters of these peripheries, as is determined by M. '*Huygens* in his *Theoremata de vi centrifugâ & motu cir-culari*; but the periphery, which a body in the equator 'describes, has its semi-diameter equal to the radius or semi-'diameter of the earth, and in any other place, the parallels 'in which bodies move, have the co-sines of their latitude 'for the semi-diameter; and therefore it will follow, that 'the force of gravity is to the centrifugal force in a pro-'portion, compounded of the radius to the co-sine of the 'radius to the co-sine of the latitude, and of 289 to 1; and 'therefore, at the Lat. of 51° 46′ (for instance) it will be as '466 to 1.

'But we must observe, that it does not from thence follow, 'that a body in that Lat. loses $\frac{1}{466}$ part of its absolute gravity, 'which it would have, were the earth at rest. For, that 'could not be, unless the centrifugal force acted directly con-'trary to the force of gravity, which it doth no where, but 'at the equator; for, let the circle Q P E (Fig. 10.) represent 'the earth, Q E the diameter of the equator, O its center; 'and let B represent a body, which we suppose to hang by 'the thread A B, and to be placed any where between the 'pole P and the equator Q; and let B D be drawn perpendi-'cular to the axis. It it plain, that if the earth had had no 'diurnal rotation, the body B would draw the thread A B 'into the position of A C; since by that means it descends as 'near as it can to the center; and there it would stretch the 'thread with all the force of its gravity; or if we will suppose 'that the centrifugal force acted, according to the same direc-'tion A C, it would then directly oppose the force of gravity, 'and the thread would remain in the same position, but it 'would be stretched with a force proportional to the diffe-'rences of these two forces: But because the body B turns

'round

'round the center D, it will endeavour to recede from it 'according to the line CB, in which direction the centrifugal 'force acting, it will not directly oppose the force of gravity; 'but it will draw the thread from the position AC into the 'position AB; let BG be drawn perpendicular to AC; if 'BC represent the centrifugal force, acting according to the 'direction BC, it is equivalent (as is commonly known) to 'two forces, one of which is as GC, and acts according to 'the direction CG, which is contrary to that by which it 'descends to O; the other is as GB, and acts according to 'the direction GB, which is no ways contrary to the force of 'gravity. If therefore, BC represent the total centrifugal 'force of the body B, that part of it, which directly opposes 'the force of gravity, will be GC: Whence it follows, that 'the decrease of gravity in going from the pole to the equa-'tor, is always as the square of the co-sine of the latitude; 'for, draw BH, parallel to the axis PP; and because the 'triangles HCB, CDO are equiangular; therefore HC is 'to CB, as CO is to CD, or as QO is to CD; but QO is 'to CD, as the decrease of gravity at Q is to the centrifugal 'force at C; and therefore, HC is to CB as the decrease of 'gravity at Q is to the centrifugal force at C: But if CB 'represent the centrifugal force at C, GC will represent that 'part of it, which acts directly against the force of gravity; 'and consequently, the decrease of gravity at the equator is 'to the decrease of gravity at C, as HC is to GC: Now, 'HC is to GC in a duplicate ratio of HC to CB, or of CO or 'OQ to CD by *Eucl.* 8. 6. and therefore, the decrease of 'gravity at Q is to the decrease of gravity at C, as the square 'of CO is to the square of CD, Q. E. D.

'Whence it is plain, that if HC represent the decrease of 'gravity at the equator, and GC its decrease at C, then will 'GH represent the difference of these two diminutions, or 'the difference between the gravity at Q and the gravity at 'C; but HC is to HG in a duplicate ratio of HC to HB, or 'of OC to DO; that is, the decrease of gravity at the 'equator is to its increase at C, as the square of the radius is 'to the square of the sine of the latitude.

'By this it will likewise appear, that the direction of heavy 'bodies is not to the center of the earth, as has been always 'suppos'd; for, if we take a heavy body and hang it by a 'thread, the thread produced will not pass thro' the center 'any where, but at the poles and the equator; for, in the

'figure, the thread is carried by the centrifugal force of the 'body B, from the position AC into the position AB, where 'it will rest.

'Now, to determine the angle CAB, which the line of 'direction of the body makes with the line AC, let AN be 'drawn parallel to BC, and produce OB, till it meet with it 'in N, and let us consider the body B, as drawn by three 'powers, according to three different directions BO, BL, 'and AB; the power, which pulls it, according to BO, is 'its gravity; that which draws it, according to the direction 'BL, is its centrifugal force; and that which acts according 'to AB is the strength of the thread, by which the body is 'hinder'd to move, according to either of the other two 'powers: But by a *Theorem*, which is demonstrated by 'several of the writers of mechanics; but particularly, by 'M. *Huygens* in his small treatise *de potentiis per fila trahentibus*; if a body be pull'd by three different powers, 'which are in *æquilibrio* with one another, according to three 'different directions AB, BL, and BO; these three powers 'will be, as the three sides of the triangle ABN, *viz.* as 'AB, AN, and BN respectively, or as AB, BC and AC; 'BN being very near parallel, and consequently, equal to 'AC, since they do not meet, but at a great distance. Hence 'it follows, that the force of gravity is to the centrifugal 'force, as AC to BC. But a method has been already 'shewn, how the proportion of the force of gravity to the centrifugal force may be determined; and therefore, the proportion of AC to BC may be also determined, which at 'the Lat. of 51° 46′ is as 466 to 1. Therefore, in the triangle ABC, the proportion of AC to BC is known, and 'the angle ACB being equal to the angle COQ, which is 'subtended by the arch CQ, the latitude of the place; from 'thence by the tables of sines and tangents, the angle BAC 'may be known, which in the above-mentioned Lat. is about 'five minutes.

' 'Hence also it will appear, that it is not the line AC, 'which being produced, passes thro' the center; but the line 'AB that is perpendicular to the curve PQ: For, all the 'particles of the fluid will settle themselves in such a position, 'that all their lines of direction downwards, must be perpendicular to the surface of the body which they compose; 'for, otherwise the parts of the fluid would not be in equilibrio with one another, and therefore, tho' the lines of direction

'tion of heavy bodies do not paſs thro' the center of the earth; 'yet are they ſtill perpendicular to their horizons; and upon 'this account there could ariſe no error in levelling of lines, 'and in finding the riſings and fallings of the ground.

'Upon this account alſo it will appear, that the ſurface of 'of the earth is not ſpherical: For, if it were, then would 'all lines, drawn from the center, be perpendicular to the 'ſurface of the earth; ſince it is the known property of a 'ſphere, that they muſt be ſo: But I have already ſhewn, 'that it is not ſo in the earth; and therefore, it is plain, that 'the earth is not a ſphere. That, therefore, I may enquire 'more particularly into the figure of the earth I will reſume 'my former hypotheſis, *viz.* that the earth is compos'd of an 'infinnite number of canals, which communicate with one 'another at the center, and are equiponderant; of which we 'will conſider two, as OQ and OC; and let OQ be $= r$, 'OD $= x$ and DC $= y$; let the abſolute gravity be call'd p, 'and the centrifugal force at the equator n. OC is '$= \sqrt{x^2 \times y^2}$ the weight of the canal; OQ is equal to the abſo-'lute gravity of the whole canal minus the centrifugal force 'of each particle in it; and becauſe the centrifugal force of each 'particle is, as its diſtance from the center; and therefore, it 'increaſes in an arithmetical progreſſion, the greateſt of which 'is n; conſequently, the ſum of all the centrifugal force is 'equal to the $\frac{1}{2} nr$: But upon the hypotheſis, that gravity is 'the ſame at all diſtances from the center, the abſolute 'gravity of the canal OQ is pr; and therefore, its real 'weight upon the center OQ is $pr - \frac{1}{2} nr$: After the ſame 'manner the abſolute gravity of the canal OC is $p \times \sqrt{x^2 + y^2}$; 'but the ſum of all the centrifugal forces of all the fluids in 'the canal OC is equal to the centrifugal force of the fluid 'in CD (as may be eaſily prov'd from the conſideration of 'inclin'd planes) but the centrifugal force at C being to the 'centrifugal force at Q, as CD is to OQ (that is as y is to r) 'the centrifugal force at C will be equal to $\frac{ny}{r}$; and be-'cauſe the centrifugal force of each particle is, as its diſtance 'from the point D, which is the center of the circle that the 'fluid in the canal CD deſcribes; and therefore, the centri-'fugal forces, in reckoning from the point D, muſt increaſe 'in an arithmetical progreſſion, the greateſt of which is $\frac{ny}{r}$;

 'and

and therefore, the sum of all the centrifugal forces in CD, 'must be equal to $\frac{nyy}{2r}$; therefore, the weight of the 'canal OC is $= p\sqrt{x^2 + y^2} - \frac{1}{2}\frac{nyy}{r} = pr - \frac{1}{2}nr$; which 'equation expresses the nature of the curve, made by the 'section of the earth with a plane thro' its poles; and by this 'the proportion of the axis of the earth to the diameter of 'the equator may be easily determined: For, when CO coin'cides with OP, then CD or y becomes equal to nothing; 'and the equation is $p\sqrt{x^2} = pr - \frac{1}{2}nr$, or $px = pr - \frac{1}{2}$ 'nr; and therefore, by *Eucl.* 16. 6. p has the same propor'tion to $p - \frac{1}{2}n$, that r has to x, or OQ to OD; but p is to '$p - \frac{1}{2}n$, as 289 is to 288 and $\frac{1}{2}$, or as 578 is to 577; which 'therefore, is the proportion of the greatest diameter of the 'earth, to the least: But this is on supposition, that gravity 'is the same at all distances from the center; but if we will 'suppose, that the gravity of bodies without the earth, is in 'a reciprocal ratio to the squares of their distances from the 'center; the gravity of those bodies, which are within the 'earth, will be directly as their distance; both which do best 'agree with the observ'd phænomena of nature; then will 'the gravity at the equator be to the gravity at the poles; as '689 to 692; which numbers, in this hypothesis, do also 'express the proportion of the diameter of the earth, drawn 'thro' its poles, to its diameter drawn in the plane of the 'equator.

'It is upon account of this diminution of gravity, according 'as we approach the equator, that pendulum's of the same 'length in different latitudes, take different times to perform 'their vibrations; for, because the accelerating force of 'gravity is less at the equator than under any parallel; and 'under any parallel it is still less than under another, which 'is nearer the poles; thence it plainly follows, that a body 'placed in the equator, or in any other parallel, will take a 'longer time to descend thro' an arch of a given circle, than 'it would do at the poles; and the farther a body is removed 'from the poles, the longer time it will take to descend thro' 'any given space.

'Hence it follows, that the length of pendulums, which 'perform their vibrations in equal times in different lati'tudes, are directly, as the accelerating forces of their gravi'ties: For, the time, a body takes to descend thro' the

'arch

'arch of a cycloid, is to the time it will take to fall thro' the 'axis of the cycloid, always in a given proportion, *viz.* as the 'ſemi-periphery of a circle is to its diameter, by the 25th 'prop. of *Huygens*'s *Horologium oſcillatorium*; and therefore, 'when the times in which a body deſcends thro' the axes of 'two different cycloids are equal, the times of the deſcent thro 'the cycloids will be alſo equal; but when the times of the 'deſcent thro' the axes are unequal, theſe axes, and conſe-'quently, the lengths of the pendulum, which vibrates in 'theſe cycloids, are proportional to the accelerating forces of 'their gravities.

'By this if we know the length of a pendulum, which per-'forms its vibrations in a given time, in any one part of the 'earth, it is eaſy to determine the length of a pendulum, 'which performs its vibrations in the ſame time, in any 'other part of the earth: As for example, the length of a 'pendulum, which vibrates ſeconds at *Paris*, is three foot 'eight lines and half; let it be requir'd to find the length of 'a pendulum, which vibrates ſeconds at the equator; becauſe 'the gravity at the poles is to the gravity at the equator, as '692 is to 686; therefore, the decreaſe of gravity at the 'equator is $\frac{3}{692}$ parts of the whole gravity: But, as I have 'before demonſtrated, the decreaſe of gravity at the equator 'is to its increaſe in any other Lat. as the ſquare of the radius 'is to the ſquare of the ſine of the Lat. Now, the lati-'tude of *Paris* being 48° 45′, its ſine is 75, 183; and 'therefore, the ſquare of the radius is to the ſquare of 'the ſine of the latitude. as 1000000 to 565248: But as '1000000 is to 565248, ſo is 3,000, the number which 'repreſents the decreaſe of gravity at the equator, to '1,695, the number which repreſents its increaſe at *Paris*; 'which added to 689, the gravity at the equator, makes '690,695, the number which will repreſent the gravity 'at *Paris*. But I have already ſhewn, that as the gra-'vity at *Paris* is to the gravity at the equator, ſo is the length 'of a pendulum, which vibrates ſeconds at *Paris*, to the 'length of a pendulum that vibrates ſeconds at the equator, 'that is, as 690,695 to 689, ſo is 36,708, the length of a pen-'dulum at *Paris*, which performs its vibration in a ſecond, to '36,616; which, therefore, is the length of a pendulum, which 'performs its vibrations in a ſecond at the equator: So that the 'difference between theſe two pendulums is $\frac{92}{1000}$ parts of an 'inch, which comes pretty near the obſervations of M. *Richer*, 'who at the iſland of *Cayenne*, whoſe Lat. is 5°, found, that

'that a pendulum which vibrates seconds there, was a tenth
'part of an inch shorter than a pendulum, which vibrates se-
'conds at *Paris*.

'Thus we see, that the principles and hypothesis, and withal 'their consequences, upon which the broad spheroidical figure 'of the earth is founded, do exactly agree with observations; 'and therefore, there is no doubt to be made; but that the 'earth is really of such a figure; and that the hypothesis, upon 'which this figure is founded, (*viz.* the diurnal rotation of the 'earth, and consequently, the centrifugal force of all bodies 'upon it) must be admitted for a true one: Since the different 'vibrations of pendulum's of the same length in different lati-'tudes, can depend upon no other cause; for, the change of air 'cannot produce any such effect. For, if the air really caused 'any alterations in the vibrations of a pendulum, it would pro-'duce a quite contrary effect to what is observed; for, pendu-'lum's near the equator would move faster than they would do 'in places of greater Lat. the air in the one place, being more 'rarified is much thinner and finer than it is in the other; and 'therefore, gives less resistance to bodies that move in it.

'In this reasoning, we have supposed the earth to have been 'at first fluid, as the *Theorist* has done before us; but if we 'will put the case, that the earth was first partly fluid and 'partly dry, as it is at present; yet because we find, that the 'land is very near of the same figure with the sea (only raised 'a little higher, that it might not be overflowed) composing 'with it the same solid; and I have already shewn, that the 'surface of the ocean is spheroidical and not spherical, there is 'no doubt to be made, but that the land was formed into the 'same figure, by its wise Creator at the beginning of the world: 'For, if it were otherwise, then would the land towards the 'equator have been overflowed with water; which, as I have 'alread proved, must have been higher at the equator than at 'the poles; and therefore, the sea would rise there, and spread 'itself like an inundation upon all the land.'

Dr. *Desaguliers* comes next to compare the experiments and observations, made use of to confirm each of the abovementioned opinions.

To prove M. *Cassini*'s figure of the earth; we must take the altitude of a star nearer than to two seconds; because two seconds answer to 32 toises on the surface of the earth, and the difference of the length of degrees is but 31: And what is more, we must take this angle with an instrument of 39 inches radius

radius; because the 10 foot sector was only used at the ends of the two parts of the meridian.

To disprove M. *Cassini*'s hypothesis; we need only observe, whether a plumb-line forms an angle of five minutes with a perpendicular to the surface of stagnant waters, or lines of level.

To prove M. *Cassini*'s hypothesis, the height of a great many mountains must be accurately measured by trigonometry, which mathematicians have always found very difficult.

To prove Sir *Isaac Newton*'s hypothesis; we are only to measure about one tenth of an inch in a rod of 39,129 inches; and to know what to allow for the lengthening of the same rod by the summer heat, when it is shut up in a case, and carried towards the equator. For, tho' the experiments on pendulum's made by several persons that travelled southwards, differ among themselves, yet they all agree in this, that the observers were obliged to shorten their pendulum's, in order to make them swing seconds, as they went towards the equator. And when we come to compare them together, in order to have the exact proportion of length in different latitudes, we must rely on the most exact experimenter, which we may very well do on M. *Richer*; because when he found a difference, he was so careful to find out how much it was, that he caused a simple pendulum to swing, and compared it with a good pendulum-clock, which he did several times every week for 10 months together; and when he returned to *France*, he compared it with the length of the pendulum at *Paris*, which is of 3 feet 8 lines and $\frac{3}{5}$ (or 39,129 *English* inches) and found it to be shorter by 1 line and $\frac{1}{4}$.

An Essay on the Natural History *of* Whales, *with a particular Account of the* Ambergris, *found in the* Sperma ceti Whale; *by Mr.* Paul Dudley. Phil. Trans. N° 387. p. 256.

THE following account relates only to such whales, as are found on the coast of *New England*; and of these there are divers sorts.

The right, or whale-bone whale is a large fish, measuring 60 or 70 feet in length, and very bulky, having no scales, but a soft fine smooth skin, no fins; but only one on each side, from 5 to 8 feet long, which they are not observed to use; but only in turning themselves, unless while young, and carried by the dam on the flukes of their tails; when with those fins they clasp about

about her ſmall, and ſo hold themſelves on. This fiſh, when firſt brought forth, is about 20 feet long, and of little value, but then the dam is very fat. At a year old, when they are called *ſhort-heads*, they are very fat, and yield to 50 barrels of oil; but by that time the dam is very poor, and called a *dry-skin*, and will not yield more than 30 barrels of oil, tho' of a large ſize. At two years old, they are called *ſtunts*, being ſtunted after weaning, and will then generally yield from 24 to 28 barrels. After this, they are called *ſcul-fiſh*, their age not being known; but only gueſſed at by the length of the bone in their mouths. The whale-bone, ſo called, grows in the upper jaw on each ſide; and is ſometimes 6 or 7 feet in length. A good large whale has yielded 1000 weight of bone. It is ſuppoſed by ſome, that the hairy part of the whale-bone and which is next the tongue ſerves for a ſtrainer of the food.

A whale's eye is about the bigneſs of an ox's, and ſituated in the hinder part of the head on each ſide, and where the whale is broadeſt; for, his head tapers away forwards from his eyes, and his body tapers away backwards; his eyes are more than half way his depth, or neareſt his under-part; juſt under his eyes are his two fins abovementioned, he carries his tail horizontally, and with that he ſculls himſelf along.

The intrails of this whale are made and ſituated much like thoſe of an ox, and their ſcalps are ſometimes found covered with thouſands of ſea-lice. One of theſe whales has yielded 130 barrels of oil, and near 20 out of the tongue. The whale-bone whale is the moſt valuable, excepting the *ſperma ceti* whale.

The ſcrag-whale is near a kin to the fin-back; but inſtead of a fin upon his back, the ridge of the hinder part of his back is ſcragged with half a dozen knobs; he comes neareſt the right whale in figure and for quantity of oil; his bone is white, but will not ſplit.

The fin-back whale is diſtinguiſhed from the right whale, by having a large fin on his back from two foot and ½ to four foot in length, whence he has his name; he has alſo two ſide-fins, as the whale-bone whale; but much longer, meaſuring 6 or 7 feet. This fiſh is ſomewhat longer than the other; but not ſo bulky; much ſwifter, and very furious, when ſtruck, and held with very great difficulty; their oil is not near ſo much, as that of the right whale, and the bone of little value, being ſhort and knobby: The belly of this whale is white.

The bunch or hump-back whale is distinguished from the right whale, by having a bunch standing in the place, where the fin does in the fin-back. This bunch is as big as a man's head, and a foot high, shaped like a plug pointing backwards. The bone of this whale is not of much value, tho' somewhat better than the fin-back's. His fins are sometimes 18 foot long and very white; his oil as much in quantity as that of the fin-back. Both the fin-backs and hump-backs are shaped in reeves lengthwise from head to tail on their bellies and sides, as far as their fins, which are about half way up their sides.

The *sperma ceti* whale is a fish much of the same dimensions with the other, but of a greyish colour; whereas the others are black: He has a bunch on his back like the hump-back; but then he is distinguished by not having any whale-bone in the mouth; instead of which there are rows of fine ivory teeth to each jaw, about 5 or 6 inches long. They are a more gentle kind of fish than the other whales, and seldom fight with their tails; but when struck, usually turn up their backs and fight with their mouths. The oil, which is made of the body of this fish, is much clearer and sweeter than that of the other whales.

The *sperma ceti* oil, so called, lies in a large trunk about 4 or 5 foot deep, and 10 or 12 foot long, near the whole breadth, depth and length of the head, in the place of the brains, and seems to be the same, and disposed in several membranous cells, and not covered with a bone, but a thick gristly substance below the skin, thro' which they dig a hole and lade out the clear oil; not but that the head, and other glandulous parts of this fish will make the *sperma ceti* oil; but the best, and that which is prepared by nature, is in the aforesaid trunk. And an ingenious person, who had himself killed several of these whales, assured Mr. *Dudley*, that the trunk alone will yield from 10 to 20 barrels: Besides the *sperma ceti* oil, this fish will yield from 20 to 50 barrels of common oil.

They propagate much like our neat cattle; and therefore, they are called bull, cow and calf. They bring forth but one at a time, and but every other year. When the cow takes bull, she throws herself upon her back, sinking her tail, and so the bull slides up; and then she clasps him with her fins: A whale's pizzle is 6 foot long, and at the root is 7 or 8 inches in diameter, and tapers away, till it come to about an inch in diameter: His *testes* would fill half a barrel; but his genitals are not open or visible, like those of the true bull. The calf or young

 whale

whale has been found perfectly formed in the cow, when not above 17 inches long, and white; yet when brought forth, it is usually 20 feet, but of a black colour; it is supposed they go with young about 9 or 10 months, and they are very fat in that time, especially, when they bring forth. When the female suckles her young, she turns herself almost upon her back, upon the rim of the water; she has 2 teats of 6 or 8 inches long, and 10 or 12 inches round. The milk is white, like that of a cow; and upon opening a young sucking whale, the milk was found curdled in his bag, just like that of a calf.

Their care of their young is very remarkable; they not only carry them on their tails and suckle them; but often rise with them for the benefit of the air; and however they are chas'd or wounded, yet as long as they have sense, and perceive life in their young, they will never leave them, nor will they then strike with their tail; and if, in their running, the young one lose his hold and drop off, the dam comes about and passing underneath, takes it on again. And therefore, care is taken by such as kill these mate fish (as they are called) only to fasten the calf; but not to kill her, till they have first secured the cow. For as soon as ever the calf is dead, the cow perceives it and grows so violent, that there is no managing her.

The whales are very gregarious, being sometimes found a hundred in a scull, and they are great travellers. In the fall of the year, the whale-bone whales go westwards, and in the spring they are headed eastwards. But here it is to be noted, that the several kinds of whales do not mix with one another, but keep by themselves.

Their way of breathing is by two spout-holes in the top of the head. The *sperma ceti* whale has but one, and that on the left side of the head. Once in a quarter of an hour, when not disturbed, they are observed to rise and blow, spouting out water and wind, and to draw in fresh air: But when pursued, they will sometimes keep under water half an hour or more; tho' it be observed, when any cow has her calf on her tail, she rises much oftener for the young one to breathe, without breathing herself; out of their breathing holes they spout great quantities of blood, when they have received their death wound.

For the first year they all suck the dam. After they are weaned, the right whales, as is generally supposed, live upon some ouzy matter, which they suck up from the bottom of the sea. The triers, that open them when dead, informed Mr. *Dudley*, that they never observed any grass, fish, or any other sort of

of food in the right or whale-bone whale; but only a greyish soft clay, which the people call *Bole Armoniac*; and yet an experienced whale-fisher told him that he had observed this whale in still weather, skimming on the surface of the water, to take in a sort of reddish spawn or brett, as some call it, that at some times will lie upon the top of the water, for a mile together. Here also it may be observed, that tho' the body of the whale is so very bulky, and so exceeding fat; yet when cut open, they are seldom found to have much more draught than that of an ox, and they dung much as neat cattle do. Their swallow is not much bigger than an ox's; but the fin-back whale has a larger swallow; for, he lives upon the smaller fish, as mackarel, herring, *&c.* great sculls of which they run thro'; and with a short turn, cause an eddy or whirlpool, by the force of which, the small fish are brought into a cluster; so that this fish, with open mouth, will take in some hundreds of them at a time. The *sperma ceti* whale, besides other fish, feeds much upon a small fish, that has a bill; the fishermen call them squid fish. The small pieces of these squid bills are plainly to be discerned in the ambergris, and may be picked out of it; they appear glazy, and resemble little pieces of broken shells.

Mr. *Harris* in his *Bibliotheca Navigantium*, &c. has given a very particular account of the method of taking whales at *Greenland*; and tho' the way in *New England* differ very much from that; yet Mr. *Dudley* waves it; only he takes notice of the boats their whale-men use in going from the shore after the whale; they are made of cedar clap-boards; and so very light, that two men can conveniently carry them; and yet they are 20 foot long, and carry six men, *viz.* the harponeer in the fore-part of the boat, four oarmen and the steersman; these boats run very swift; and by reason of their lightness can be brought on and off; and so kept out of danger: The whale is sometimes killed with a single stroke; and yet at other times she will hold the whale men in play, near half a day together, with their lances; and sometimes will get away, after they have been lanced and spouted blood, with irons in them and drugs fastened to them, which are thick boards about 14 inches square. The people in *New England* formerly used to kill the whales near the shore; but now they go off to sea in sloops and whale-boats, in *May*, *June* and *July*, between *Cape Cod* and *Bermudas*; where they lie by in the night; and sail to and again in the day, and seldom miss of them; they bring home the blubber in their sloops. The true season for taking the right or

whale-bone whale is from the beginning of *February* to the end of *May*; for the *ſperma ceti* whale, from the beginning of *June*, to the end of *Auguſt*. And it has been obſerved by the fiſhermen, that when a *ſperma ceti* is ſtruck, he uſually, if not always, throws the excrements out of the *anus*.

The prodigious ſtrength of this animal lies principally in the tail, that being both their offenſive and defenſive weapon. Mr. *Dudley* had ſeveral inſtances of this kind from credible perſons, who were eye witneſſes, a few of which are, as follows; a boat has been cut down from top to bottom with the tail of a whale, as if cut with a ſaw, the clap-boards ſcarce ſplintered, tho' the gunnel upon the top be of tough wood. Another has had the ſtem or ſtern poſt of about three inches thro', and of the tougheſt wood that can be found (into which the ends of the cedar clap-boards are nailed) cut off ſmooth above the cuddee, without ſo much as ſhattering the boat, or drawing the nails of the clap-boards. An oar has been cut off with a ſtroke upwards, and yet not ſo much as lifted up out of the thole-pin. One perſon had an oar cut off, while in his hand; and yet he never felt any jarring.

A few years ſince, one of the fin-back whales came into a harbour near *Cape Cod*, and towed away a ſloop of about 40 tun, out of the harbour into the ſea. This accident happened thus; it was ſuppoſed the whale was rubbing herſelf upon the fluke of the anchor, or going near the bottom got the fluke into her niſket or the orifice of the *uterus*, and finding herſelf caught, tore away with ſuch violence, that ſhe tow'd the ſhip out of the harbour, as faſt as if ſhe had been under ſail with a good gale of wind, to the aſtoniſhment of the people on ſhore; for, there was no body on board. When the whale came into deep water, ſhe went under, and had like to have carried the ſloop with her; but the cable gave away, and ſo the boats that were out after her, recovered her. This whale was found dead ſome days after, upon that ſhore, with the anchor ſticking in her belly.

After a whale has been dead, it has been obſerved, that the ſame way the head lies, ſo the head will lie if not forcibly turned; and let the wind blow which way it will, that way they will ſcull-a-head, tho' right in the eye of the wind; and they are much eaſier towed to the ſhore, if they die with their head that way, than any other.

The fiſh that prey upon the whales, and which often kill the young ones, (for they will not venture upon an old one, unleſs much

much wounded) are by the whale-men called killers; these are from 20 to 30 foot long; and have teeth in both jaws that lock within each other. They have a fin, near the middle of their backs, four or five foot long. They go in company by dozens, and set upon a young whale, and will bait him like so many bull-dogs; some will lay hold of his tail to keep him from threshing, while others lay hold of his head, and bite and thresh him; till the poor creature, being thus heated, lolls out his tongue; and then some of the killers catch hold of his lips, and if possible, of his tongue; and after they have killed him, they chiefly feed upon the tongue and head; but when he begins to putrify they leave him. This killer is undoubtedly the *orca*, that Dr. *Frangius* describes in his treatise of animals to the following purpose, 'when an *orca* pursues a whale, the latter makes a terrible bellowing, like a bull when bit by a dog.' These killers are of such vast strength, that when several boats together have been towing a dead whale, one of them has come and fastened his teeth in her, and carried her away in an instant down to the bottom; and sometimes they have bit out a piece of blubber of about two foot square, which is of that toughness, that an iron with little beards being stuck into it, will hold it, till it draw the boat under water. The killers are sometimes taken and make good oil, but have no whale-bone. The carcases of whales in the sea serve for food for gulls, and other sea-fowl as well as sharks.

Many and various have been the opinions even of the learned world, as to the origin and nature of ambergris: Some have reckoned it a *bitumen*, and to issue from the bowels of the earth; others, that it was produced from some insect, as honey, silk, &c. The famous Mr. *Boyle* communicated in a former *Transaction* an account of ambergris from a *Dutch* merchant, who first denies it to be the scum or excrement of a whale; and then gives it, as his opinion, that it is a fat gum that issues from the root of a tree, and that you may raise it in quantities, by planting those trees by the shore; and so the stream will cast it up to great advantage. But it is now found, that this *occultum naturæ* is an animal production, and bred in the body of the *sperma ceti* whale, analogous to what is found in some land-animals, as the musk-hog or *taiacu*, the musk-deer, the bezoar sheep, and some amphibious animals, as the musquash, &c. who have their valuable scent in a particular *cystis* or bag. Mr. *Dudley* is apt to think, that what first gave occasion to the notion of ambergris being the production of the whale was, because

cause it was found in considerable quantities on the shore of the *Summer Islands* and among the *Babama*'s, where the dead whales are frequently wreck'd and broke up with the sea, and the ambergris found floating, or on the shore: But here again the ingenious, till very lately, were at a loss and divided in opinion; for, tho' they agreed that it came from the whale; yet some took it to be the true and proper *semen*, being found only in the bull, at the root of the *penis*, near the *testes*; others again took it to be the ordure or excrement of the whale.

The best and most exact account Mr. *Dudley* could procure, was from one Mr. *Atkins*, at *Boston* in *New England*, who used the whale fishery for 10 or 12 years together; and was one of the first, that went out a fishing for the *sperma ceti* whales, about the year 1670, and then he began to discover the ambergris; his account, and which agreed with that Mr. *Dudley* had from several whale-men was, as follows.

'The ambergris is found only in the *sperma ceti* whales, and 'consists of balls or globular bodies, of various sizes, from 'about 3 inches to 12 inches in diameter, and will weigh from 'a pound and a half to twenty two pounds, lying loose in a 'large oval bag or bladder, of three or four foot long, and 'two or three foot deep and wide, almost in the form of an ox's 'bladder, only the ends more acute, or like a blacksmith's 'long bellows, with a spout running tapering into, and thro' 'the length of the *penis*; and a duct or canal, opening into the 'other end of the bag, and coming from the kidneys; this 'bag lies just over the *testes*, which are upwards of a foot long, 'and it is placed lengthwise at the root of the *penis*, about 'four or five foot below the navel, and three or four foot above 'the *anus*. This bag or bladder is almost full of a deep 'orange-coloured liquor, not quite so thick as oil, and smelling 'strong, or rather stronger, of the same scent with the balls of 'ambergris, which float in it; the inside of the bag is very 'deeply tinged with the same colour; the liquor may also be 'found in the canal of the *penis*; the balls seem to be pretty 'hard, while the whale is alive; inasmuch as there are fre'quently found, upon opening the bag, large concave shells, of 'the same substance and consistence, that have scaled off from 'them; and the balls themselves seem to be composed of se'veral distinct coats, inclosing each other, something like the 'coats of an onion.'

As to the number of balls, Mr. *Atkins* never found above four in a bag; and in a bag where he found one that weighed

21 pounds

21 pounds (which was the largest he ever saw) there was no other.

Mr. *Atkins* farther affirms, 'that to one *sperma ceti* whale 'that has any of these balls, there are two, that have nothing 'but the aforesaid deep orange-coloured liquor in their bags.' This remark confirms what another whaleman told Mr. *Dudley*, *viz.* 'that the ambergris was found only in such *sperma ceti* 'whales, as are old and well grown.'

It is the general opinion of the whalemen, that the ambergris is only produced by the male or bull *sperma ceti* whale. As to this particular, Mr. *Atkins* affirms, 'he never saw nor heard 'of a *sperma ceti* female taken in his life; the cows of that 'species of whales being much more timorous than the bulls, 'and almost impossible to be come at; unless they happen to 'be found asleep on the water, or detained by their calves.' This is certain, that the boats can never come near them, when they are awake, they are so very shy and timorous.

Mr. *Atkins*'s method of getting the ambergris out of the whale was thus; after the fish is killed, he turns the belly upwards and fixes a tackle to the *penis*, then cuts a hole round the root of the *penis*, thro' the rim of the belly, till he come to the intrails; and then searching for the duct or canal at the farther end of the bag, he ties it pretty near the bag, and cuts the duct off beyond it; upon which he draws forth the *penis* by the tackle, and the ambergris bag entirely follows it, and comes clean and whole out of the belly.

Mr. *Prince* of *Boston* (who took the preceeding relation from Mr. *Atkins*) takes the aforesaid bag to be the urinary bladder, and the ambergris ball to be a certain concretion, formed out of the greasy odoriferous substance of the aforesaid liquor, contained within it.

An enquiry into S. Valsalva's *Discovery of an excretory* Duct *from the* Glandula renalis; *by Mr.* John Ranby. Phil. Trans. N° 387. p. 270.

MR. *Ranby* procured a human body in order to search for this duct, which he did with all possible diligence: He was not so happy as to discover any duct of this kind; but having injected the *aorta*, he found the arteries, going to the *glandula renales*, disposed, as represented in Fig. 11. Plate XIII.

Now whether that branch of the artery, which goes down from the *glandulæ renales*, on both sides towards the *testes*, without supplying any of the neighbouring parts, might not have

have been miſtaken by the learned anatomiſt *S. Valſalva* for an excretory duct (all arteries in dead bodies being generally free from blood, and of a whitiſh colour) Mr. *Ranby* will not determine, for want of farther experiments.

A A (Fig. 11.) repreſents the right kidney; B B the left kidney; C C the deſcending trunk of the *aorta*; D the right emulgent artery; E the left emulgent artery; F the right *glandula renalis*; G G the right ſpermatic artery; H the left *glandula renalis*; II the left ſpermatic artery; K K the *ureters*; *a a* a ſmall artery ariſing out of the deſcending *aorta*, a little above the right emulgent artery: It ſends two branches *b b* upwards to ſupply the right *glandula renalis*; a third branch *c c* goes downwards towards the right ſpermatic artery GG; and then it farther accompanies the ſame to the right *teſtis*; *d d* a ſmall artery ariſing out of the left emulgent artery, near the deſcending trunk of the *aorta*, and going directly upwards to the left *glandula renalis* H; *e e* a ſmall artery ariſing out of the deſcending trunk of the *aorta*, a little below the left emulgent artery; it divides into two branches; one of which *ff* goes upwards between the emulgent artery and vein to the left *glandula renalis*; the other *g g* downwards, towards the left ſpermatic artery, and accompanies it to the left *teſtis*.

The Sequel *of the* Diſſertation *on the* Figure *of the* Earth; *by Dr.* Deſaguliers. Phil. Tranſ. N° 388. p. 277.

DR. *Deſaguliers* met with a diſſertation of M. *Mairan* (publiſhed in the *Memoirs of the Royal Academy of Paris* for the year 1720) wherein the learned and ingenious author has taken a great deal of pains to reconcile the obſervations made on pendulums (found to be ſhorter at the equator than at *Paris* when they ſwing ſeconds) with the oblong ſpheroidical figure of the earth, deduced from M. *Caſſini*'s meaſures. And tho', upon a ſtrict examination of his conjectures, and what he gives for demonſtrations, Dr. *Deſaguliers* does not find reaſon to alter his opinion, concerning the oblate or flatted ſpheroid, which Sir *Iſaac Newton* has ſhewn to be the figure of the earth: Yet ſince it might be thought by ſome who have read M. *Mairan*'s treatiſe, and afterwards may read the Dr's, that the latter has not conſidered all the circumſtances which the former has done, and that he has not been exact enough in the mathematical part of his diſſertation; becauſe he has drawn ſome concluſions from ſuppoſing the figure of the earth ſpherical, when he ſhould

ſhould have ſuppos'd it an oblong ſpheroid; the Dr. ſhews here, wherein he thinks M. *Mairan* is miſtaken, and to give additional proofs of his aſſertions in *Phil. Tranſ.* N° 386. 387.

Firſt then the Dr. begins with the conjectures.

M. *Mairan* affirms, that it is as reaſonable to ſuppoſe the earth (if it was once fluid) to have been an oblong ſpheroid at firſt, as a ſphere; and that in ſuch a caſe, the centrifugal force of the ſeveral parts of the earth, ariſing from its revolution about its axis, which might convert a ſphere into an oblate ſpheroid, would only change an oblong ſpheroid into one leſs oblong.

If the earth were at firſt a fluid (ſuppos'd homogeneous and of any given form) and left to thoſe laws, which we find to obtain at preſent, it muſt put on a ſpherical figure, for the ſame reaſon that drops of mercury, of water and other fluids, put on ſuch a figure. And to ſuppoſe any change made in that figure from the preſſure of an external fluid, filling up all ſpace, is contrary to what has been demonſtrated by Sir *Iſaac Newton* in his *Principia lib. 2. prop.* 19. Where he ſhews, 'that if any portion of a fluid be compreſs'd by the 'ſame or any other homogeneous fluid, that portion will not 'have its figure alter'd by that preſſure.'

And indeed, we ſee, that in the receiver of the air-pump, lumps of butter, coagulated oil or honey, drops of quickſilver or water, &c. have the ſame figure, whether the preſſure of the air act upon them, or be taken off by exhauſting the receiver.

That a fluid ſubſtance of any figure will by the gravity of its parts become ſpherical, is plain by the following demonſtration.

Let ABCDE (repreſented Fig. 12. Plate XIII.) be a portion of a homogeneous fluid, whoſe parts tend towards each other, and whoſe figure is not ſpherical. If in ſuch a fluid we ſuppoſe a ſyphon, as ACE (or which is the ſame thing, if all the fluid ſhould be frozen, excepting the canal ACE) whoſe legs AC and CE are unequal, and meet at C, the center of the fluid, towards which there is the greateſt tendency: the fluid will run out at A in the leg AC, till it be come down as far as *g* in the leg CE, ſuppoſing C*g* equal to AC. But if the leg AC be lengthened as far as *c*, then the fluid will only come down, as far as *e* in the leg CE, and

at the ſame time riſe up to *a* in the leg C*a*, C*a* being equal to C*e*.

If ſuch another canal or ſyphon be ſuppos'd at BCD, the fluid in it will come down from D to *d*; and riſe from B to *b*. And ſince ſuch ſyphons may be ſuppos'd all over the fluid ABDE, that fluid by the mutual tendency of its parts towards each other, muſt be reduced to the ſpherical figure *abde*. Q. E. D.

Now, without conſidering the unreaſonableneſs of the ſuppoſition, let us imagine the earth to have been an oblong ſpheroid at firſt, and then to have a diurnal rotation given it, which ſhould by degrees ſhorten its axis, to bring it to what M. *Caſſini* and M. *Mairan* ſuppoſe it at preſent to be. If in ſuch a caſe the earth be ſuppoſed fluid enough to change its figure, by the rotation round its axis, why ſhould it ſtop when the equatorial diameter comes to want $\frac{1}{96}$ part of the length of the axis? Since two powers, *viz.* gravity andthe centrifugal force act upon it to ſhorten its axis; the firſt of theſe has already been ſhewn capable to reduce it to a ſphere, and the centrifugal force is acknowledged by M. *Mairan* to be (as Sir *Iſaac Newton* has prov'd it) equal at the equator to $\frac{1}{289}$ part of the gravity there. Certainly the alteration of figure would not have ſtopp'd, before the earth came to be a ſphere; nay, and it muſt have riſen at the equator; and how much, Dr. *Deſaguliers* has already ſhewn in *Phil. Tranſ.* N° 386. 387.

Again, if we ſuppoſe the earth compos'd of a heterogeneous fluid, before the diurnal revolution, the heavieſt parts would tend towards the center, and the lighter towards the ſurface, and that way the terraqueous globe would alſo become a ſphere. Then if, when the central forces are fix'd, and the ſuperficial *ſtrata* fluid, the earth receive a diurnal motion; it will riſe at the equatorial parts, and that to a greater height than what the Dr. has ſhewn in the ſaid *Tranſactions*, where he ſuppos'd the earth, compos'd of uniform matter. And that ſomething like this muſt be the caſe, appears from what Sir *Iſaac Newton* has ſaid upon this ſubject. For, after having ſhewn, from ſuppoſing the earth, compos'd of uniform matter, that the centrifugal force of all its parts would bring it to be 17 $\frac{1}{6}$ *Engliſh* miles higher at the equator than at the poles, and after having given a table of the proportionable decreaſe of the length of the degrees of a meridian

meridian of the earth, going from the poles to the equator in ſuch a figure of the earth with the lengths that pendulum's muſt have to ſwing ſeconds in ſeveral latitudes; from a comparison of the lengths of pendulums (obſerv'd by different perſons to be ſhorter towards the equator, than in greater latitudes (when they ſwing ſeconds) he ſhews, that the earth muſt be $31\frac{7}{12}$ miles higher at the equator than at the poles; and therefore, that it muſt be denſer towards the central than the ſuperficial parts to produce a flatted ſpheroid, where the equatorial diameter muſt exceed the axis ſo much more, that is, be longer ſomething more than $\frac{1}{125}$ part.

Dr. *Deſaguliers* is very well aware, that it may be objected by ſuch as have read M. *Mairan*'s diſſertation, and have not read Sir *Iſaac Newton*'s *Principia*, or not with due attention, that the Dr. has not argu'd fairly, in drawing conſequences from a greater gravity at the equator than at the poles, in an oblong ſpheroid; becauſe M. *Mairan* has ſhewn, that in ſuch a figure of the earth, the gravity is greater at the poles than at the equator; and that the Dr. ſhould have drawn his conſequences from theſe principles. To this the Dr. anſwers, that M. *Mairan*'s demonſtrations about gravity are built upon wrong ſuppoſitions, as ſhall be ſhewn anon. Nevertheleſs, ſuppoſing gravity to be greater towards the poles than towards the equator, in the proportion he aſſigns; namely of the ray of curvature, drawn into the perpendicular to the curve, terminated at the axis; let us conſider what will follow from his principles.

Let us then ſuppoſe the earth at firſt in a fluid ſtate; A A (as repreſented Fig. 13.) the axis; *d* Æ the equatorial diameter; *ab* a ray of curvature; *dn* another; *ac* and *d* C two lines of tendency, or perpendiculars to the curve, intercepted by the axis at *c* and C; and *d* C, A C two tubes or canals of the fluid, gravitating towards and communicating at C; the Dr. ſays, that according to M. *Mairan*'s principles of gravity, the earth cannot retain its oblong ſpheroidical figure: For, ſince the gravity at *a*: is to the gravity at *d* :: as $dn \times d\,C$: to $ab \times ac$; it will follow (from the nature of the ellipſis) that the gravity at A : will be to the gravity at *d* :: as AC^4 : to dC^4 : and therefore, the forces, with which the columns of fluid A C and *d* C tend towards C, will be as their maſſes, drawn into the forces, driving towards C, that is, as $AC \times AC^4$ to $dC \times dC^4$. Now by the principles of hydroſtatics, it is evident, that the fluid, in the canal A C, will cauſe the fluid in

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the canal dC to run out at d, as long as $AC \times AC^4$ is greater than $dC \times dC^4$; and if the canal Cd be continued quite to δ, the surface of the fluid in AC will sink to α, whilst the surface of the fluid in dC rises up to δ; in which case as $\alpha C = C\delta$, the point A will come to α, and the point d to δ, and the curve Ad being changed into $\alpha\delta$, the oblong spheroid will be changed into a sphere, the only figure consistent with the *equilibrium* of the fluid parts, according to M. *Mairan*'s own principles; because then you will have $AC^4 = dC^4$, and $AC \times AC^4 = dC \times dC^4$. If we make use of Sir *Isaac Newton*'s principles in this reasoning, we shall also shew, that an oblong, spheroidical, fluid earth will be changed into a sphere; but not so fast, as it does by M. *Mairan*'s laws: For, according to Sir *Isaac Newton*, the gravity at A: is to the gravity at d : : as $\sqrt[5]{Cd} : \sqrt[5]{AC}$. Q. E. D.

Here we have suppos'd no diurnal revolution; for, as soon as that begins, the centrifugal force will raise the equatorial parts, and change the sphere into a flatted spheroid; as has been shewn before, and is allow'd by M. *Mairan*.

Now, if we suppose the same figure of the earth, but the land (at its first creation) as firm as it is now; it will in that case follow from M. *Mairan*'s principles, that the sea must rise and overflow all the equatorial regions, tho' the earth had no diurnal revolution; and much more so, when the centrifugal force, arising from the diurnal motion, helps to carry the water the same way.

Demonstration. Let $P\,æ\,P\,Æ$ Fig. 14. represent the plane of a meridian; PP the axis of the earth (suppos'd an oblong spheroid) $æ\,Æ$ the diameter of the equator; $d\,e\,a\,æ$ part of the surface of the earth; $æA$ and eB two perpendiculars to the surface of the earth (which are here two rays of curvature) fc the surface of the sea, and $f\,d\,e\,g$, $b\,a\,æ\,c$ two cylinders of sea water of equal bases and equal heights.

Since gravity acts on the unequal columns of water $b\,a\,c\,æ$; $f\,d\,e\,g$ in the reciprocal ratio of the ray of curvature (at the respective places of the columns) drawn into that part of it, which M. *Mairan* calls the line of tendency (that is, in the ratio of $eB \times eZ$ to $æA \times æC$) the weight of fe: will be to the weight of $bæ$: : as $æA \times æC : eB \times eZ$; therefore, if there be a communication between the fluid columns fe and $bæ$, there cannot be an *equilibrium*, till the quantity of matter in fe be to the quantity of matter in $bæ$, reciprocally

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as the gravity at the place $æ$ is to the gravity at e; and in that case the height ge will be reduced to ke; if $ke : cæ :: eB \times eZ : æA \times æC$; and consequently, the surface of the sea will go thro' the points $ikbc$, where bc under the equator is higher than ik towards the poles, Q. E. D.

N. B. That the centrifugal force will still add to the height of the sea at bc, is plain from what was said before. And if we apply these principles to determine the different lengths of pendulum's, swinging seconds at *Paris* and at the equator; from the gravity at *Paris*, compared with the gravity at the equator (in this supposition of the action of gravity and figure of the earth) a pendulum must be shorter at the equator by more than 10 lines, without considering the centrifugal force and if the centrifugal force be taken into the account, the pendulum's must be shortened near a whole inch. But this being about five times more than what agrees with observation; what proves too much, proves nothing at all.

Having thus shewn, that M. *Mairan*'s account of the action of gravity, on several places upon the surface of the earth, can be of no service for reconciling the experiments made on pendulum's, with the figure of the earth, deduced from M. *Cassini*'s measures; Dr. *Desaguliers* proceeds to shew, that his demonstrations are founded upon wrong principles; and first in relation to gravity.

This Gentleman has followed Sir *Isaac Newton* in saying, that gravity increases in a duplicate reciprocal ratio of the diminished distance from the center of the force, and so *vice versâ*; but he has followed Sir *Isaac Newton* no farther than served his present purpose; otherwise he would have known — That with respect to a central body, as a planet towards which others are attracted or impelled by gravity, this law obtains only, as bodies attracted are removed from the surface of the planet, to greater distances from the center, compared with that distance, or as from greater distances they approach nearer to the planet — That the greatest action of gravity is at the surface of the planet — That afterwards in advancing towards the center, the force of gravity on the body attracted, becomes continually less, decreasing directly as the distance; and that this holds true in a spheroid as well as in a sphere — That on different parts of the surface of the earth (in the condition it is now) the gravity on bodies is reciprocally, as their distance from the center of the earth — That tho' at a considerable distance we look upon the earth, or any planet, or even the sun,

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as a point (in the center of the forces tending towards it) endued with an abſolute force, proportional to its quantity of matter; yet when we come ſo near the body, as to conſider the ſpace it poſſeſſes, we are to take notice, that the whole attraction or gravity of the body is made up of the ſum of the attraction of all its parts, properly combined; and therefore, that when a corpuſcle, or body attracted, comes to be within the planet or body attracting, the matter above it draws it back in ſuch a manner, that it leaves it only a force to go on towards the center, which is directly as the diſtance, as has been ſaid already: Juſt as if a body concentric to the planet (whether ſpherical or ſpheroidal) had its ſurface juſt where the corpuſcle is, and all the exterior cruſt or ſhell were annihilated.

The Dr. does not doubt, but M. *Mairan* will be of this opinion, when he has carefully and impartially examined the 12th and 13th ſections of Sir *Iſaac Newton's Principia*, and the 18th, 19th and 20th *Prop.* of the third book. And if he will be at the pains to compare the 38th and 39th *propoſitions* of the third book with the 66th of the firſt, he will find, that the preceſſion of the equinoxes is owing to the broad ſpheroidical figure of the earth; and that if it had M. *Caſſini*'s figure, the equinoctial points would move *in conſequentia* faſter than they do now *in antecedentia*.

Farther M. *Mairan* demonſtrates, that in an oblong ſpheroid, the diminution of gravity by the centrifugal force, increaſes faſter in going from the poles to the equator, than it would do in a broad ſpheroid; and therefore would ſhew, 'that notwith-'ſtanding the ſurface of the earth is nearer to the center in 'M. *Caſſini*'s figure than in Sir *Iſaac Newton*'s; yet the centri-'fugal force will diminiſh the gravity ſo faſt in going from *Pa-'ris* to the equator, that the ſhortening of pendulums, to make 'them ſwing ſeconds at the equator, may very well be accounted 'for that way.'

Now let us examine into this matter, to ſee whether the cauſe be adequate to the effect.

If the diſtance from the ſurface of the earth at the pole to the center be 96, and the diſtance of the ſurface at the equator be 95, the diſtance of the ſurface at *Paris*, in the Lat. of 48° 50′, will (by the property of the ellipſis) be 95,562, *&c.* Now ſince the force of gravity, in different places on the earth's ſurface, is reciprocally as the diſtance from the center; and the lengths of pendulum's, that perform their vibrations in the ſame time, are directly as the force of gravity; therefore, the

length of pendulums at *Paris* will be to their length at the equator, as 95 to 95,562, &c. that is, as 440,555, &c. to 443,165, &c. and consequently, they must be lengthened 2, 61, &c. lines. But as from M. *Mairan*'s principles, the diminution of gravity by the centrifugal force, is greater at the equator than at *Paris*, hardly $\frac{1}{440}$ part of the whole gravity at the equator, the pendulum's must be shortened in that proportion; so that then the length of second-pendulum's will be 440,555 + 2,61 — 1 lines. But as that quantity is greater than 440,555, &c. therefore, the pendulum's upon the whole must be lengthened: Nay, tho' we should allow a shortening of two lines; since by observation pendulum's are found to be about two lines shorter at the equator, the oblong spheroidical figure of the earth cannot be consistent with the experiments on pendulum's.

Dr. *Desaguliers* here sets down M. *Mairan*'s aforesaid demonstration, in order to see, whether he has assumed true principles, vide M. *Mairan*'s dissertation, article 11, &c.

Propos. 5. 'The centrifugal force at any degree of latitude, 'taken upon the oblong spheroid, between the equator and the 'pole, is less in comparison to the centrifugal force at the equa- 'tor, than it would be at the same degree of latitude, taken 'upon a sphere, or which is the same thing, the centrifugal 'force increases more, going from the poles towards the equator, 'upon an oblong spheroid, than upon a perfect sphere; and 'consequently, gravity diminishes more, and a pendulum must 'be more shortened under the equator, in the hypothesis of the 'oblong spheroid, than in that of a perfect sphere.

'Having described an oval curve of any kind, as for example, 'the abovementioned ellipsis ADBE (Fig. 15.) and inscribed 'the circle DHE, whose radius is DC = half the shorter axis 'DE; upon AD take any point as R, between the equator and 'pole; and from that point to the *evoluta* OTX draw the ray 'of curvature RT, which gives the line of tendency RP '(Art. 4.) likewise draw from the common center C, to the 'circumference of the circle DH; a radius CV, parallel to 'PR, and meeting the circle at V; then from the points RV, 'draw the lines RN, VZ, perpendicular to the axis AB.

'It must be observed, first, that as the ellipsis AD represents 'a meridian of the oblong spheroid, the circle DH represents 'a meridian of a sphere in the same plane.

Secondly,

'Secondly, that the point V on the circular meridian, an- 'swers to the same degree of latitude, as the point R upon the 'elliptical meridian; because the lines P R, C V, being paral- 'lel to each other, and perpendicular, the one to the ellipsis and 'the other to the circle (by construction) the touching planes, or 'horizons of the points R V, will likewise be parallel.

'Thirdly, hence it follows, that the diminution of the cen- 'trifugal force (acting against gravity) on account of its obli- 'quity to the horizon (Art. 10.) of the same degree of latitude 'on the elliptical and on the circular meridian, is the same in 'both cases, and in the same ratio, as the absolute centrifugal 'forces, represented by the perpendiculars R N, V Z (Art. 9.) 'therefore, to know whether the centrifugal force (either abso- 'lute or relative) of the point R, upon the oblong spheroid 'A D B E be less or greater with respect to the centrifugal force 'under the common equator D E, than the centrifugal force '(whether absolute or relative) of the correspondent point V 'upon the sphere; nothing more is required than to see, which 'is the longest of the two perpendiculars, namely, R N in the 'oblong spheroid, or V Z in the sphere: Since these two lines 'express the *radii* of the circles of revolution; and conse- 'quently, the absolute quantity of the centrifugal forces.

'Fourthly and lastly, that the ratio of the centrifugal forces 'of two correspondent points upon the oblong spheroid A D B E 'and the inscribed sphere D H E, to the centrifugal force of 'their equators is the same, supposing the sphere of any other 'bigness; and that it has been determined here of the diameter 'D E, only to render the demonstration easier, by giving the 'same consequent to the antecedents R N and V Z. For, if 'about the center C and with the radius C *d*, the circle *d h e* be 'described equal, for example, to a meridian of a sphere of the 'same solidity, as the oblong spheroid A D B E; and the ra- 'dius C V be produced, till it meet the circle *d h* at the point *u*, 'and *u z* be let fall perpendicular to the common axis of revo- 'lution, and parallel to V Z. It is plain, that we shall always 'have $VZ : DC :: uz : dC$; or $\frac{VZ}{DC} = \frac{uz}{dC}$; and conse- quently, $\frac{RN}{DC}$ will have the same ratio to $\frac{UZ}{DC}$ as to $\frac{uz}{dC}$.

'Therefore, in order to demonstrate, that the centrifugal 'force of a point, taken in any latitude upon the oblong sphe- 'roid is less when compared to the centrifugal force of the like 'point,

'point, taken upon a ſphere, with reſpect to the centrifugal 'force at the equator; there is nothing more required than to 'ſhew that $RN < VZ$; becauſe by that means we ſhall have

$$\frac{RN}{DC} < \frac{VZ}{DC}.$$

'This being obſerved; from the point R, draw the line RI, 'parallel to the axis AB, and meeting the circle DH at K, 'and the diameter DE of the equator at the point I: From 'the point K having let fall the perpendicular $KL = RN$, 'upon the axis AB, and drawn KC to the center C; the queſ'tion will be brought to this, *viz.* to know whether the point 'V coincides with the point K; or whether it is above it to'wards D, or below towards H.

'But $CK = CV = CD > PR$ (Art. 8.) therefore, CK 'and PR being both between the parallels AC, RI, the 'greateſt CK is more inclined to them than the leaſt PR; and 'the angle KCA is leſs than the angle $RPA = VCA$: And ſince 'theſe two angles have each of them one of their ſides, coin'ciding with the line AC, namely, the ſide AP of the angle 'RPA, and the ſide AC of the angle KCA; it follows, that 'the ſide VC of the angle $VCA = RPA > KCA$, will go 'above CK between CK and CD, and meet the line RI at 'the point G, between K and I, and the circle DH at the 'point V, which conſequently will be above RI, between K 'and D: Therefore, $CV = CG + GV$ is $= PR + GV$; 'and conſequently, VZ, which meets RI at the point F, is '$= ZF + FV = RN + FV$; and therefore, $RN = VZ$ '$- FV$; therefore $RN < VZ$.

'And becauſe the ſame thing may be demonſtrated with re'ſpect to any other point, taken between the equator and the 'pole; and that gravity, and conſequently, the length of a 'pendulum diminiſhes, as the centrifugal force increaſes. 'Therefore, &c. Q. E. D.

'*Corollary* 12. From what has been demonſtrated, and 'from *Prop.* 3. Art. 8. it follows, that the perpendicular, 'which is drawn from any point of an oval meridian to the 'axis, will be ſo much ſhorter, in compariſon to the perpendicu'lar drawn from the correſpondent point of an inſcribed circular 'meridian, as the latitude is greater; and conſequently (by 'Art. 11. N° 3.) the centrifugal force will be ſo much the leſs, 'and gravity ſo much the greater, upon the oblong ſpheroid, 'with reſpect to the centrifugal force, and the gravity under the 'equator.

'For, as the line RP does always decreaſe, as the point R 'is taken nearer the pole A; it is evident, that the angle VCK 'will continually increaſe, with reſpect to the angles VCA, 'KCA, as it is their difference; and conſequently, that the 'perpendicular VZ will be ſo much greater than the perpendi-'cular KL = RN.'

The Dr. omits the demonſtration of the latter part of M. *Mairan*'s abovementioned propoſition, which he juſtly deduced from his conſtruction, if what he ſays (N° 2.) be right; becauſe in ſuch a caſe it cannot be called in queſtion; and proceeds to an obſervation he makes afterwards, *viz.* 'We muſt take care to 'obſerve in the foregoing propoſitions and corollaries, that the 'compariſon is always made between two ſimilar points of lati-'tude, taken upon the two ſpheroids, or upon one of the ſphe-'roids and the ſphere, between the equator and the poles, with 'reſpect to the centrifugal force upon the equator of any one of 'theſe ſpheroids or of the ſphere. For, if we only compared 'abſolutely the centrifugal force of a point of the equator of 'the one, to the centrifugal force of a correſponent point of the 'equator of the other; it is plain, that it would be greater upon 'a flatted ſpheroid than upon a ſphere, or than upon an oblong 'ſpheroid of the ſame ſolidity, in the ratio of the great axis of the 'generating ellipſis of the flatted ſpheroid, to the diameter of the 'ſphere, or to the ſhorter axis of the generating ellipſis of the 'oblong ſpheroid. And in all likelihood, this muſt be the rea-'ſon that has made others, who have treated of this ſubject to 'imagine the very contrary of what I have demonſtrated.'

As M. *Mairan* conſiders the earth at reſt, in the conſtruction for his above quoted demonſtration, and afterwards obſerves what effect the centrifugal force will have upon bodies on its ſurface, to diminiſh the gravity, with which they endeavour to deſcend in their line of tendency RP: He ſhould not only have taken notice (as he has done) that the whole centrifugal force NR is not to be ſubſtracted from the gravity at R, as the whole centrifugal force CD is to be ſubſtracted from the whole gravity at D, becauſe of the obliquity of RN to PR; but he ſhould have alſo obſerved, that the obliquity of the plane of the parallel NR, in which the centrifugal force acts, muſt alter the line of tendency RP, and change the direction RP into RW, ſomewhere between the point P and the center C; for, if there be a heavy body as a plummet, hanging by a thread in the line SR or SP, the line of tendency which has been ſuppoſed perpendicular to the curve ARD, without taking in the effect of the centrifugal force; as ſoon as the ſpheroid revolves about its axis,

axis, the body which would fall in the line S R, acted upon only by one force, namely, that of gravity, will now be acted upon by another force, at the same time pushing it in the line Ss (which is the same as Rr) and consequently will move in the line Sr, diagonal of the parallelogram $sSRr$; or which is all one, a body placed at R will have its line of tendency in R W, as the Dr. has already shewn in *Phil. Transf.* N° 386; only he did not suppose the earth a spheroid before the diurnal motion; and therefore, made use of the line Z V, instead of the line N R: So that it may be objected, that the angle rSR will not be so great in a spheroid, as in a sphere; because the centrifugal force, which acts with the same obliquity (since N R P = Z V C) is as much less in the spheroid, as N R is less than Z V. But the Dr. was aware of this; and therefore, he made the angle RSr only of five minutes, when it really appears to be almost six, when the earth is supposed spherical; and therefore, without coming to give the exact quantity of the said angle, one may easily perceive, that M. *Cassini*'s difference of the axis and equatorial diameter will produce a figure, in which the angle RSr will not be less than five minutes.

Such an obliquity, caused in the direction of gravity, will render the oblong spheroidical figure of the earth impossible; because then fluids would not have the lines of their gravity, perpendicular to the horizons of the places, where they are (supposing the horizons of places to be planes, touching the curve of the earth in those places) and plumb-lines would be so far out of the perpendicular to lines of level, as to make an angle easy to be observed, as the Dr. has shewn before.

But if the same cause be supposed to act upon the sea to make it level, as makes heavy bodies to fall (which certainly must) then indeed, lines of level will be perpendicular to plumb-lines, and the level of the sea, taken always for the horizon of a place, will not be a plane touching the earth, but cutting it towards the poles, and consequently, the water will be carried towards the equator, as was before shewn.

Besides, the difference of the action of the centrifugal force would not be so great between correspondent points of the same latitude in the spheroid and in the sphere: For, when the line of tendency R P is by the centrifugal force changed into R W, the point R upon the spheroid does no longer correspond in latitude with the point V upon the sphere; but must be taken nearer to V; so that the line R W may become parallel to V C, and R W A = V C A.

If it be alledged here, that M. *Mairan* ſuppoſes the earth in motion, and takes in the effect of the centrifugal force, when he makes the line of tendency to be R P; to this Dr. *Deſaguliers* anſwers, that if M. *Mairan* had conſidered the earth, as revolving upon its axis, he would not have made V C, the line of tendency of a ſpherical earth in motion; ſince it is the line of tendency of ſuch an earth at reſt.

In M. *Mairan*'s obſervation abovementioned, he ſays, ‘ that ‘ we are not to compare the centrifugal force at the equator of ‘ an oblong ſpheroid, with the centrifugal force at the equator ‘ of a ſphere, or at the equator of a flatted ſpheroid of the ‘ ſame ſolidity; allowing that then it would be greater in the ‘ ſphere, and ſtill greater in the flatted ſpheroid: But only the ‘ centrifugal forces in ſeveral latitudes upon the ſame figure.’ Dr. *Deſaguliers* differs from him for the following reaſons.

1 Becauſe the force of gravity is not the ſame at the equator of the flatted ſpheroid, as it is at the equator of the ſphere, or as it is at the equator of the oblong ſpheroid.

2. Becauſe it is not the ſame in different latitudes, in either of the ſpheroids (vide Sir *Iſaac Newton's Princip. lib.* 3. *Prop.* 19, 20.) And M. *Mairan*'s way of arguing will only ſerve, in caſe the gravity ſhould be the ſame in all the points of the ſurface of the earth in his figure, and alſo in the other two figures.

For example, let the uniform gravity be called g; and

1. Let the centrifugal force at the equator of the flatted ſpheroid be called $c + 2$, and the centrifugal force in any latitude, as for inſtance, the latitude of *Paris* (as it is diminiſhed on account of a ſhorter co ſine of latitude, and likewiſe on account of its obliquity to the line of tendency) be called $c + 2 - l$ the difference of the diminution of gravity at *Paris*, and at the equator will be $\overline{g - \overline{c+2}} - \overline{g - \overline{c+2-l}} = l$.

2. Let the centrifugal force at the equator of the ſphere be called $c + 1$, and the centrifugal force at the latitude of *Paris* be called $c + 1 - \overline{l + m}$; the difference of the diminution of gravity at *Paris*, and at the equator in a ſpherical earth, will be $\overline{g - \overline{c+1}} - \overline{g - \overline{c + 1 - 1 - \overline{l+m}}} = l + m$.

3. Let the centrifugal force at the equator of the oblong ſpheroid be called c, and the centrifugal force at *Paris* be called $c - \overline{l + m + n}$, the difference of the diminution of gravity

gravity at *Paris*, and at the equator, in an oblong ſpheroidical earth, will be $g - c - \overline{g - \overline{c - \overline{l + m + n}}} = l + m + n$.

Now, if gravity ſhould in every caſe be equal to g; it is evident, that the ſhortening of pendulum's at the equator would be greater in the oblong ſpheroid than in the ſphere, or in the flatted ſpheroid; becauſe as the lengths of pendulum's diminiſh with the gravity, thoſe lengths will be at *Paris* and at the equator, when compared, as $g - \overline{c + 2 - l}$ to $g - \overline{c + 2}$, in the flatted ſpheroid, as $g - \overline{c + 1 - \overline{l + m}}$ to $g - \overline{c + 1}$ in the ſphere; and as $g - \overline{c - \overline{l + m + n}}$ to $g - c$ in the oblong ſpheroid; and conſequently, from what M. *Mairan* has demonſtrated, this ratio of $g - \overline{c - \overline{l + m + n}}$ to $g - c$, being greater than either of the others, the pendulum's muſt be ſhortened in the oblong ſpheroid.

But as the force of gravity is leſs at the equator of the flatted ſpheroid, than at the equator of the ſphere, or of the oblong ſpheroid of the ſame ſolidity; let us expreſs its quantity in the three caſes by $g - s$, g, and $g + s$, and we ſhall then find the lengths of the pendulums, at the equator of the three ſolids, as $g - s - \overline{c + 2}$, $g - \overline{c + 1}$, and $g + s - c$; and conſequently, the lengths of pendulum's will be greateſt at the equator of the oblong ſpheroid, becauſe $g + s - c$ is the greateſt quantity.

Laſtly, to compare the lengths of pendulum's at the equator of the oblong ſpheroid, thus found, with their lengths at the latitude of *Paris* upon the ſaid ſpheroid. Let us expreſs the exceſs of gravity at the equator, whereby it is greater than at *Paris* (becauſe in this figure, *Paris* is farther from the center of the earth than the equator by $\frac{1}{190}$ part) by the letter s, and the exceſs of the centrifugal force at the equator, above that part of it which acts directly againſt gravity at *Paris*, by $l + m + n$, the gravity at *Paris* by g, and the centrifugal force at the equator by c; then $g + s - c$ will ſtill repreſent the diminiſhed gravity, and anſwer to the length of pendulum's at the equator, whilſt $g - \overline{c - \overline{l + m + n}}$, or $g - c + l + m + n$ repreſents the diminiſh'd gravity; and conſequently the length of pendulum's at *Paris*. If s be equal to $l + m + n$, pendulum's will be as long at the equator, as at *Paris*; and if s be greater than $l + m + n$, pendulum's will be longer at the equa-

equator. But making all possible allowance, in favour of M. *Mairan*'s hypothesis, no calculation will bring $l + m + n$ to be greater than, or ever equal to s: Therefore, his demonstrations, abovementioned, are of no force to prove the earth an oblong spheroid.

And now, Dr. *Desaguliers* thinks, he has answered all that relates to the figure of the earth in M. *Mairan*'s dissertation; by shewing, that his conjectures can neither be supported by those physical principles, which Sir *Isaac Newton* has mathematically deduced from unquestioned observations and experiments, accurately made; nor even by those principles M. *Mairan* himself has assumed to serve his intended purpose — That his demonstrations, relating to the difference of the action of the centrifugal force, are of no service to him, for reconciling the experiments made on pendulum's, with M. *Cassini*'s measures; because when applied to Sir *Isaac Newton*'s principles, they will make pendulum's longer at the equator than at *Paris*; and when applied to M. *Mairan*'s own principles, they will make them a whole inch shorter at the equator than at *Paris*, contrary to all observations, which at a medium make pendulum's, but about two lines or $\frac{166}{1000}$ part of an inch longer at the equator than at *Paris* — That he has built his demonstrations upon a wrong notion of gravity — And that he has not considered what is most material in the effect of the centrifugal force, acting on bodies descending by their gravity, between the equator and the poles; namely, the alteration of their line of direction, which would make them fall out of the perpendicular towards the equator.

Dr. *Desaguliers* adds one more philosophical argument, communicated to him by a friend, and which is entirely independent on those principles of philosophy, concerning which, some of the Gentlemen that believe the oblong spheroidical figure of the earth and the *English* philosophers, are not yet agreed; and it is this.

If the earth were of an oblong spheroidical figure, higher at the poles than at the equator; the axis of its revolution would either go thro' one of its short diameters, or be continually changing, unless the axis did exactly coincide with the axis of the figure.

Demonstration. Suppose such an oblong figure as A *a* (Fig. 16.) fixed to the axis P *p* at the center C, but capable of moving freely round it towards P or towards *p*; yet so as to be obliged to move with the axis, when it is turned round. Suppose now the poles P and *p* to be fixed; and the body, thus con

conſtituted, to be turned ſwiftly round the axis P*p*; then if the angle ACP be oblique, and the figure AD*a*E be oblong, the parts AC and C*a* will acquire a centrifugal force, which will enlarge the angle *p*CA, till it comes to be a right one. Beſides this, a velocity will be generated in the motion, while A is going towards the perpendicular *a*C, which will make it go farther on towards P, as to B, with a motion which will after that be retarded, till the centrifugal force has ſtrength enough to ſend it back again the contrary way; and ſo it will move continually with a reciprocal motion, like the oſcillation of a pendulum; and if a little of this motion be loſt at every oſcillation, then the oblong figure AD*a*E will at laſt move quietly about its leſſer axis DE, coinciding with P*p*.

If A*a* did not at firſt exactly coincide at P*p*, the centrifugal force will have the above-mentioned effect; and that this is not the caſe in the earth is more than probable; becauſe the unequal diſtribution of ſea and land, beſides the phænomena of the tides muſt make the axis of its gravity, and conſequently, the axis of its revolution, to differ from the axis of the oblong ſpheroid, if the earth had ſuch a figure; without conſidering that every earthquake would alter ſo nice an *equilibrium*, which once loſt would never be recover'd again.

To leave nothing unexamined, relating to the controverſy, Dr. *Deſaguliers* again conſiders the meaſures and obſervations mentioned in the account of the meridian, drawn thro' *France*, in the *Memoirs of the Royal Academy*, for the year 1720; and he finds them to want a great deal of the accuracy, requir'd in ſo nice a point, as determining the different lengths of degrees upon the ſurface of the earth: To prove his aſſertion, Dr. *Deſaguliers* refers the reader to the following tables, by which it appears, that if any thing certain can be deduced from the ſaid obſervations and meaſures (either taken as they are, or reduced to the level of the ſea, by M. *Caſſini*'s rules, vide *Memoirs of the Royal Academy* for the year 1720 *vol.* I. *p.* I. *ch.* 13.) it will be in favour of Sir *Iſaac Newton*'s figure of the earth, rather than theirs.

In the following table, the firſt column exhibits the names of places; the ſecond the diſtances from *Paris*, according to the meaſures taken by the *French* gentlemen; the third the latitudes, ſuch as the meaſured diſtances will give them, ſuppoſing the earth ſpherical; the fifth the differences between

between these and the latitudes obſerv'd, expreſs'd in ſeconds of a degree, where, when the latitude computed exceeds the latitude obſerv'd, the letter N (north) ſhews that difference to be in favour of M. *Caſſini*'s figure, and the contrary difference, mark'd by the letter S (ſouth) is in favour of Sir *Iſaac Newton*'s figure.

Names of Places.	Diſtances from *Paris* meaſured.	Latitudes obſerved.			Latitudes in a ſpherical Earth computed from the meaſured diſtances.			Differences in ſeconds.	
I.	II. Toiſes.	III.			IV.			V.	
		°	′	″	°	′	″	″	
Dunkirk.	125552	51	2	25 ½	51	2	25 ½	o	
Amiens.	60370	49	53	48	49	53	48,	o	
Sourdon.	49970 ½	49	T	42	49	42	52,1	10,1	N
Paris.		48	50	10	48	50	20,3	10,3	N
Malvoiſine	18838	48	30	47	48	30	32,1	14,9	S
Vouſon.	67962	47	39	17	47	38	53,6	23,4	S
Bourges.	100192	47	4	31	47	04	58,7	27,7	N
S. Sauvier	139934	46	23	24	46	23	12	12,0	S
Croc.	169540	45	51	43	45	52	4,6	21,6	N
Bort.	196484	45	23	27	45	23	45,2	18,2	N
Aurillac.	223606	44	55	13	44	55	14,5	1,5	N
Rodès.	256575	44	20	54	44	20	35,1	19,9	S
Alby.	280612	43	55	32	43	55	19	13,0	S
Carcaſſone	321430	43	12	56	43	12	24,5	31,5	S
Collioure.	360604	42	31	13 ⅔	42	31	13,8	o.	

In this table it is to be obſerved, that there is an equal number of differences, marked N. and S. and if the differences on each ſide be added together, there will be 89″ 4′ on the north ſide, and 114″,7 on the ſouth. This laſt agrees beſt with Sir *Iſaac Newton*'s figure, which muſt be ſuppoſed for the correction of ſo great a difference.

In the next table, the firſt column exhibits the names of places ; the ſecond the latitudes obſerved ; the third the diſtances in the meridian from *Paris*, reduced to the level of the ſea ; the fourth the differences of the ſecond column, expreſſed in ſeconds of a degree ; the fifth the differences of the numbers in the third column ; and the ſixth the meaſure of a degree by the fourth and fifth columns compared.

Names

I. Names of places.	II.			III.	IV. Second of a degree.	V.	VI.
				Toises.		Toises.	Toises.
Dunkirk.	51°	2′	19′	125454	4103″	65010	57040
Amiens.	49	53	56	60444	1859	29416	56965
Clermont.	49	22	57	31028	1967	31028	56787
The R. Observatory.	48	50	10	0	4253	67959	57525
Vouson.	47	39	17	67959	4553	71978	56912
S. Sauvier.	46	23	24	139237	1901	29602	56058
Croc.	45	51	43	169539	1677	26941	57834
Bort.	45	23	46	196480	1713	27136	57028
Aurillac.	44	55	13	223616	2060	32858	57422
Rodés.	44	20	53	256474	1521	24138	57131
Alby.	43	55	32	280612	2557	40818	57468
Carcassone.	43	12	55	321430	2502	39184	56380
Collioure.	42	31	13	360614			

In this table in the third column, over against St. *Sauvier*, the number which was 139944 is corrected to make it 139937, in favour of the oblong spheroid. In the sixth column, the numbers appear so irregular, as to be unfit to decide this controversy. Then if a comparison be made between *Dunkirk*, St. *Sauvier* (which is very near the middle of *France*, and almost in the meridian of *Paris*) and *Collioure*, the mensuration is absolutely in favour of Sir *Isaac Newton*'s theory; the mean degree between *Dunkirk* and St. *Sauvier* being larger by about 64 toises than between St. *Sauvier* and *Collioure*; and to reduce them to an equality, there must be a greater alteration made in the situation of those three places, than is reasonable to suppose their observations are capable of admitting. Here follows the comparison.

Dunkirk and *Collioure*	a mean degree is	57061
Dunkirk and *Paris*		56960
Paris and *Collioure*		57097
Dunkirk and St. *Sauvier*		57090,4
St. *Sauvier* and *Collioure*		57026,5

According to M. *Picard*. 57060

To conclude, Dr. *Desaguliers* proposes a method of observing the shadow of the earth in lunar eclipses; whereby the

the differences between the diameters in the oblong ſpheroidical figure, if there be ſuch an one, as M. *Caſſini* affirms (*viz.* of *96* to *95*) may be diſcovered.

Let P Æ P Æ (Fig. 17.) repreſent the earth, ſeen from the ſun at the time of the ſummer ſolſtice; it is evident, that the ſame figure will expreſs the ſection of the earth's ſhadow at the moon's diſtance, as ſeen from the earth. If E E repreſent the ecliptic, Æ Æ will be the ſhorteſt diameter of the ſection; and if L L be taken for the path of the moon, in a total and central eclipſe thereof, by obſerving the time ſpent in the paſſage of the center of the moon, thro' the ſhadow, and reducing that time to ſeconds of a degree of a great circle of the heavens, we ſhall have the leaſt diameter of the ſhadow.

Again let the ſame letters (as Fig. 18.) repreſent the ſame things; only here the ſection of the ſhadow is ſuch, as the earth will caſt at the equinox, and the eclipſe of the moon is here ſuppos'd partial, its center juſt touching the ſhadow. When the moon's center is got to *c*, if the latitude of its center or its diſtance from the ecliptic be obſerv'd, we ſhall have the length *c* C nearly equal to the longeſt ſemi-diameter of the ſhadow.

Now comparing *c* C (Fig. 18.) to L C (Fig. 17.) the difference between *c* C and C P in the former figure and between C L and C Æ in the latter not being worth notice; they ought to be to each other, as *96* to *95*, which in ſuch a ſhadow will give a difference of about 25″ at a medium, ſenſible enough to be obſerv'd, notwithſtanding the *penumbra*. If therefore thoſe aſtronomers, who have inſtruments nice enough, and ſufficient ſkill in the management of them, to take angles to three or four ſeconds of a degree, will obſerve what the Dr. has been mentioning in total and partial eclipſes of the moon; by ſuch obſervations they will eaſily convince us, that the figure of the earth is ſuch, as M. *Caſſini* ſuppoſes it, or convince him that he has been miſtaken.

The ſemi-diameter of the earth's ſhadow, when the earth is in *perihelio*, and the moon in *apogæo* is 38′ or 2280″, without conſidering the increaſe of the ſhadow, on account of the earth's atmoſphere, which would make it 39′ or 2340″ (allowing one ſecond for a mile) and the ſame ſemi-diameter of the ſhadow, when the earth is in *aphelio*, and the moon in *perigæo* is 46′ 20″ or 2780″, which increas'd on account of the earth's atmoſphere, will bring it to 47′ 20″, or 2840″. Now

if

the ratio of 95 to 96 be taken in both cases you will have these analogies. $\left\{\begin{matrix} 95 : 96 :: 2340'' : 2364'',6 \\ 95 : 96 :: 2840'' : 2869'',8 \end{matrix}\right\}$ So that $2364'',6 - 2340'' = 24'',6$ will be the difference of the semi-diameters, when the section of the shadow is the least; and $2869'',8 - 2840 = 29'',8$ will be the difference of the semi-diameters, when the section of the shadow is the greatest; the sum of those differences $24'',6 + 29'',8$ halv'd, will give the difference, when the section of the shadow is at a medium $= 27'',4$: From which if we take $2'',4$, because in Fig. 18. C*c* is a little less than C P, and in Fig. 17. L C is something greater than Æ C; we shall have C*c* in Fig. 18. to compare with L C in Fig. 17. which will exceed it by $25''$, if M, *Cassini*'s figure of the earth be the true one.

The End of the SEVENTH VOLUME.

THE

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CORRIGENDA.

PAGE 355 *l.* 24. for adulcorated *r.* edulcorated. *p.* 398 *l.* 24 for this *r.* thus. *p.* 436 *l.* 33 for *crainum r. cransum. p.* 417 *l.* 40 for bolt head *r.* long neck. *ibid. l.* 42 for lid *r.* stopple. *p.* 418 for bolt-head *r.* long neck. *ibid. l.* 15. for decripitates *r.* decrepitates. *p.* 439 *l.* 10 for may *r.* might.